JOURNAL
OF THE
FRANKLIN INSTITUTE.
OF THE STATE OF PENNSYLVANIA,
FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CXVII. APRIL, 1884. No. 4.

THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the JOURNAL.

MECHANICS—INTRODUCTORY.

By Prof. COLEMAN SELLERS.

[Delivered at the opening of the course of lectures on Mechanics, Friday, November 9, 1883. Revised from report of Phonographer.]

(Concluded from page 172.)

In revising this lecture for publication in the JOURNAL OF THE FRANKLIN INSTITUTE, I cannot help giving another illustration of this uncertainty. An eminent scientist is reported as having stated that a horse-power is 33,000 lbs. raised one foot high, regardless of the time taken to raise it to that height; that 33,000 lbs. raised one foot high in a minute or in a day in each case expresses the same power. I cannot credit this and I will not do so unless the assertion comes to me in print from the hands of the author. It is so easy to misunderstand a speaker, and scientific lectures are not so correctly reported as lectures involving fewer technical terms.

So common is it to talk of the power of this or that machine as expressed by horse-power, that we feel as if its true definition should be part of every school boy's education. I think that it should be made a part of the tables of weights and measures that now form the school boy's early study. There should not be any one who could not tell, when asked, that what is known as a horse-power is a force equal to the raising of 33,000 lbs. one foot high in a minute, and that weight, distance, and time are the three essentials of horse-powers.

We can compute the power of the hoisting machines we have used in illustration this evening and determine the fraction of a horse-power a strong man can exert with either one of them, if we find what is the greatest load he can lift with the machine and the height to which he can raise it in one minute of time. The division of the weight lifted in the time named by 33,000 gives us the horse-power exerted.

In this city, noted for its great manufacturing establishments, there seems to be especial need of instruction that will fit the young of both sexes for work that will be remunerative sooner if the foundation of useful knowledge is in a shape for ready use. A well-grounded knowledge of the great law or principle of conservation of energy should be taught with the multiplication table. It can be so taught if the teachers themselves are certain that there is in the universe only so much energy and that we cannot make one particle more than already existed. With a clear understanding of this principle no time will be wasted in search after perpetual motion machines, and fewer mistakes will be made by really earnest seekers after improved machines for use or improved methods.

When a young man brings to me some wonderful improvement over the ordinary crank motion, some device that is to supersede the crank of the steam engine, a feeling of utter helplessness comes over me; I know not where or how to begin; he has had no opportunity to learn the simple laws of mechanics, and to point out the fallacy of his argument means to teach him the laws of mechanics, so I can only say to him "Don't," and may advise him what books to read.

We hear or read almost daily of the wonders of science and what is to be accomplished by electricity. "It is to be the great power of the future." Is it a power now? We may use it indirectly to drive machinery, we may make use of it to propel the cars on our street roads, but is it a power in the sense that steam is a power? Let us think of this a few moments. We call steam a power, and our factories are driven by steam power; or we call water when falling a power, and we drive the machinery in other factories by water wheels; or we pump water into the reservoirs at Fairmount by water power. Where we have no fall of water, and where fuel is scarce but wind plenty, we grind corn in a mill driven by wind, and the wind is our power; these and other sources of power may be called primary powers.

Secondary power is that which is transmitted from the prime motor

to a machine. One machine may be driven by belt power and another may be driven by gearing, etc. Electricity, as we now use it, as a power must be classed in its greatest economy with the secondary powers, with the belt or the gearing, not with the steam engine and the water wheel. We dig from the earth coal that contains the stored up energy of the sun's heat expended on forests that existed long before man came to live on this planet. We burn that coal under our boilers, and the steam generated by this application of heat to water is used to drive the piston of the steam engine and from thence is the power conveyed by belt or gearing, by shafts, or even by electricity, to the machines to be operated. We can burn up zinc in costly acids and generate electricity that can be used to drive an electric engine, and so in turn operate machines exactly as in the case of the steam engine. In this case electricity is a power exactly as steam is to be considered as a power, and what is more the electric battery will give us more nearly the whole of the stored-up energy of the metal eaten up in the battery than the most improved steam engine can give us of the stored-up energy of the coal that is devoured in the furnaces under the boilers. With all this advantage, electric batteries are not used to drive machines with any hope of economical results.

Zinc has been gathered from the earth as an ore, it has been converted into a metal, or the metal has been gathered from the ore by means of coal and much labor; its market price is measured by the cost of its production. To burn up zinc at five cents a pound in acids costing but few cents per pound, with a certainty of getting from the metal 70 or 80 per cent. of its theoretical energy in motive force, yet makes the venture a more costly one than the burning of coal under a boiler with the knowledge that we are at the best getting but little more than ten per cent. of the theoretical power that lies hidden in that coal. The electricity that is now lighting our streets, the electricity that is utilized in places to drive the street cars, has behind it the steam engine or the waterfall, the wind-mill or some other motor.

By means of a steam engine we drive a dynamo-electric machine, and the electricity thence proceeding lights our street or may be reconverted, with some loss, back into the power that created it; for one dynamo machine can be made thus to drive another, the electricity being carried from one to the other by proper conductors What then is electricity as we now use it in the way of power but as the belts and the gearing that carries our steam power to the machines? It is a

belt with more or less slip. But this is not to remain so forever. The future of electricity as a power is full of promise. The coal we now squander, using but a small percentage of its theoretical dynamic force, is capable of yielding its energy either as heat or as electricity, and the time will come when we will not burn this coal to boil water and in that boiling lose say 1,000 units of its heat at the moment of the conversion of water into steam, lose all this never to be getting it back, but we will take from the coal its energy in the form of electricity we hope in more near ratio to its true value, and then we can convert that energy into whatsoever other form of energy we may require. The best that science can do is to point out just what energy there is in this or that source of power. The most we can hope to utilize of this energy as power will never amount to 100 per cent. Nature gives us nothing without exacting something in payment.

A pound of water is the same as a pound of metal so far as its power from gravity is concerned. In falling through space it will exert just as much force as any other pound weight is capable of doing and no more; it will do the work due to one pound falling at any given velocity less the friction of the machine or of the moving parts. We turn water into steam with a certain knowledge of the power that can be gained by using the elastic vapor as a spring, or we may tear the gases, which combined form water, apart and use these gases in recombination to produce power, but less power than was taken to tear them apart, never more.

Science has made us so sure of these facts that we can base our faith on them, and with this knowledge we are willing that others than ourselves shall invest their money in machines which are claimed to be able to develop from five drops of pure water inclosed in a ball, power enough to propel the largest steamship across the ocean. It is ignorance of the unalterable laws of physics that leads ignorant people into squandering money on so-called wonderful inventions that, out of nothing, are to give us great results. An ignorant man will spend his time pondering over perpetual motion machines, so will a man with brain gone wrong; the first will quit his folly with more learning, the second finds his home in the mad-house. A third and worse class aim to deceive and, for a time, many a one has done so. When shrewd ignorance resorts to dishonest methods, the confiding public is apt to suffer in pocket. I have here a curious document which I will read to you:

WHEREAS, The interference of the Legislature of Pennsylvania in causing an inquiry to be made, relative to the perfection or imperfection of newly invented machinery, is not without precedent;

AND, WHEREAS, It has been represented that Charles Readhefer, of the County of Philadelphia, has invented a machine declared, not only by the inventor but by many intelligent persons, to possess the power of self-motion;

AND, WHEREAS, Should it be ascertained that those opinions are correctly founded, not only great honor would be conferred upon this Commonwealth, but incalculable advantages would be derived from the invention by the people of the United States especially, and by mankind in general;

AND, WHEREAS, On the other hand, should the machine be found to be imperfect, the public interest would be promoted by exposing its fallacy;

AND, WHEREAS, The Legislature of the Commonwealth reposes confidence in the integrity and qualifications of Henry Voight, Robert Patterson, Nathan Sellers and Oliver Evans, of the City of Philadelphia, Archibald Binney, Lewis Wernwag and Josiah White, of the County of Philadelphia, and Samuel D. Ingham, of the County of Bucks; therefore,

Resolved by the Senate and House of Representatives of the Commonwealth of Pennsylvania, in General Assembly met, That Henry Voight, Robert Patterson, Nathan Sellers, Oliver Evans, Archibald Binney, Lewis Wernwag, Josiah White and Samuel D. Ingham be and are hereby requested to make a strict examination of the machine invented by Charles Readhefer, and to make as specific representation respecting its alleged importance and the public expectation require.

Resolved, That the Secretary of the Commonwealth be and is hereby requested to transmit a copy of the foregoing preamble and resolution to each of the persons named therein, and also to Charles Readhefer.

JOHN TOD,
Speaker of the House of Representatives.

P. C. LANE,
Speaker of the Senate.

In the House of Representatives, December 14, 1812, read and adopted.

GEO. HECKERT,
Clerk of the House of Representatives.

In Senate, December 17, 1812, read and adopted.

JOSEPH A. McJIMSEY,
Clerk of the Senate.

Now it so happens that we have in the Franklin Institute, Isaiah

Lukins's model of this, at one time celebrated perpetual motion machine. This is it on the table at my side. I presume it a correct model of the original, except that in this, the motion is obtained by clock work concealed in the base, and Reidheifer's machine was operated by means of a crank turned by a man or boy in the room below. In this model there are glass plates below the steps of the diving and driven wheels, that these plates can be taken out is assumed to offer convincing evidence that the wheels were not connected in any manner with any source of power outside of the machine itself. One can scarce look at this machine without feeling astonishment that any one should have been deceived by the wiley man, who claimed so much for it as to warrant an examination by a commission at the instance of the Senate and House of the State of Pennsylvania in General Assembly met. This was in 1812. I have talked with many who were active men at that time, and I know that its believers were numbered by thousands. One old man told me how, meeting a fellow traveler one night as he jogged out to his home in Montgomery county, they adjourned to a wayside inn, and there his companion made from an old cigar box a model to prove that Reidheifer's perpetual motion would do what was claimed for it. The sun was beginning to show itself when they were done with the interesting argument. Listen to the argument: "A loaded wagon will run down a hill. If the hill is steep enough, and the hill is capable of moving out from under the loaded wagon, then if the wagon is prevented from moving except in a vertical direction it will push the hill from under it. Now in this so-called perpetual motion machine there are two hills or inclined plains mounted on opposite sides of a wheel, which wheel is horizontal, its axis being vertical. There are loaded wagons on the two inclined planes and as the wagons cannot go down the inclined planes, but are held by a complicated system of levers and the inclined planes cannot move from under them, therefore the effort to do what both are prevented from doing, results in a constant push and the wheel is supposed to be driven around by this power." There are many stories told about this commission's visit to see the machine. One name on the commission is that of my grandfather, and his son, my father, went with him to see it and noted that while the large wheel was supposed to drive the small one, yet the wear on the teeth was on the side of the teeth that would indicate that the small wheel was driving the big one. It is said too that Isaiah Lukins, the maker

of this model, was also present and said the jerky motion of the machine was very indicative of a crank turned by hand-power. The commission found out nothing for they were not permitted to probe too deep, but they were none the less sure that gravity without motion in its own direction can impart no motion to other parts, no matter what complicated system of devices are made to take part in the fraud.

Another model was made that has since been destroyed by fire, which model could be taken apart and examined in detail, its bearings were on glass and when it was restored, all parts in proper position, it would show no signs of motion until the weights on the little carriages were placed on them and then the machine would run. I bring this old model to your notice this evening as a reminder of how very easy it is for those who are not well grounded in the fundimental laws of mechanics to be deceived. From the time of the perpetual motion machine of the Marquis of Worcester, down past Reidheifer in a time nearer to us, there have been presented innumerable such follies as this old model shows, and the world is full to day of those who if they had the money to spend would risk it in such foolish ventures. It is even said that in this present day there is not one hundred miles from where we now are, a greater wonder in the mechanical line. I persume it is so but I have not seen it.

In concluding this lecture I wish to show you an experiment and tell you how that experiment appears, to minds differently educated. What I will presently show you, I one time used, to test the inventive faculty of two very able men. One evening, some years ago, I had a visitor in the person of one of our most noted scientists, a man to whom I should go for information in the direction of his specialty, with the greatest confidence in the correctness of his judgment. The other man was an engineer, a member of this Institute. We were talking of the possible action of the mind on matter and the first caller, the professor, related some very remarkable things that had occured in his experience with a noted medium. What he had seen, seemed to him to prove the posession on the part of the medium of some power that was extraordinary and he argued that as his own habit of thought had been trained in the exact sciences, therefore, he could not have been the victim of any deceit. Wishing to test his inventive faculty which is what is required in considering such matters. I turned his attention to the then, rather new statements of certain very noted Englishmen that there existed a force or power with some per-

sons called, by one writer on the subject, physic force. This new force being shown in the power to will the change of weight in inert matter. I do not know if there has ever been any one who assumes to possess this power, who has exercised it in the direction of influencing the weight of the butter he had to buy in market, or of enabling the scale to weigh light when selling or heavy when buying. The fact is the examples given of the exercise of the power were on the whole rather useless. In fact, we may safely say, that there does not seem to be any great good derivable from the experiments published as tried in demonstration the existence of this force. I called his attention to a piece of apparatus I had made to show this force. Here it is. You see I have a ball of black wood about four inches in diameter, it is solid and through its centre is pierced a hole about $\frac{5}{16}$ of an inch in diameter through which hole I can pass this brass rod. The ball is in fact a large wooden bead and I can string this bead on a small cord, which about fills the hole but still the ball will slide freely on the cord, as freely as on the brass rod. The closest examination will show you nothing but the ball and the cord. The cord stretched tight is like a rod in the ball and it slides back and forth over the tense cord. This I gave to the professor to examine as much as he liked, letting him become familiar with it at his leisure; then, taking it in my own hands thus, I showed him that by the exercise of the physic force which I assumed I had in common with others, the ball would no longer show the same freedom of motion on the cord, except when I willed it to do so. See, I hold the cord vertically, one end under my foot the other held above my head. The ball remains near to my hand, and as I will it to do so descends as slowly or as fast as I wish it to. It is in fact under my control. See, I tell the ball to go to half the length of its string and its stops at the place indicated. Now this experiment continued to some length in order to give my friend the best chance to discover the method used by me, to restrain the motion of the ball only convinced him of the possession on my part of this so called new force. A force that sets at defiance the laws of physics that are most sure. A force that does not imply some newly discovered attraction, but controls one long and well known, namely the attraction of gravitation. Turning to my engineer, I asked him what he thought of the experiment? He replied, that he had never seen it before, but he should infer that, in as much as the ball was free to slide no matter what tension was made on the cord when it was in the hands of the

professor, that when I manipulated I had some means of increasing its friction, whereby I could control the amont of friction. I asked him how? For instance, he said, it is quite possible for the ball to be made with two holes in it, one passing through the axis of the sphere in a right line, and a second hole with its ends terminating in the straight hole, but the course of the second hole being made curved or crooked, then when the cord was threaded through the curved or crooked passageway, the friction could be controlled by the more or less tension of the cord. This was the true solution of the trick, for trick it was and is. A trick perhaps familiar to many of you, but interesting as a very clever device for testing the inventive faculty of those who see it for the first time.

Apart from the mechanical problems involved in the apparatus there is of course required the skill of hand, that enables the cord to be threaded into either one of the two passages, without any seeming hesitation in the simple act of threading the ball into the cord. There is a great deal of superstition hiding in all our minds and we are too apt to credit with some supernatural significance occurrances that are simple enough when we come to see them in the proper light. The training of the young should be more in the direction of a development of the inventive faculties, not to make an inventor of the grown up child, but to teach him how to view all things dispationately.

Lectures can do but a small point of this instruction, books must do more, and the public schools must be made to do their full share of the work of training, the hands, as well as the head. Under the wise direction of the present superintendent of public schools, we look for much good in this direction. It is our earnest hope that the present course of lectures may not only interest, but serve to illustrate what should be taught.

Cleansing Old Paintings—M. de Bibra begins by removing the dust with a brush, and washing with a sponge dipped in pure water. The surface of the picture is then covered with a thick layer of soap, which is removed after eight or ten minutes by a hard brush and water. After the last traces of soap are removed the picture is thoroughly dried, and rubbed with linen rags dipped in nitro-benzine. The cleansing is complete when the rags cease to be soiled. After again drying, the painting is covered with fine olive oil and varnished. —*Chron. Industr.*, Oct. 7, 1883. C.

INITIAL CONDENSATION OF STEAM CYLINDERS.

By William Dennis Marks,

Whitney Professor of Dynamical Engineering, University of Pennsylvania.

(Concluded from page 183.)

Possibly the greater economy of actual steam which it is claimed uniformly results from superheated steam lies in the certainty of *dry* saturated initial steam, the greater certainty of more complete re-evaporation during expansion, and the presence of less water on the interior walls of the steam cylinder demanding re-evaporation during exhaust than would occur with saturated steam.

Practically, superheating is done with the waste gases of combustion, and therefore cost nothing for fuel. Because what is called saturated steam is not always dry, we should not jump to the conclusion that under all circumstances it is the best plan to use superheated steam.

It may be the case with superheated steam that part of it (the main body) retains a greater specific volume than saturated steam.

Let us see what this increase of specific volume is in the case of the Harris Corliss engine.

We can say, with sufficient accuracy for our purpose at seven atmospheres.

	Pressure abs. Pounds.	Temp. Fahr. Deg's.	Specific volume. Vol's.
Saturated steam	102·9	329·6	265
Superheated steam	102·9	532	344
Difference			79

Making an increase of volume of 30 per cent. in the saturated steam, or a diminution in weight of about 20 per cent., volume for volume; but we must recollect that upwards of 50 per cent. of the weight of initial steam was condensed in the actual experiment on the Harris Corliss engine, and therefore if all the steam uncondensed in the steam cylinder at cut off be assumed to be superheated, the saving in weight would be much less than 20 per cent. of the actual weight of steam used.

Nor have we any guarantee that more than a small portion, if any, of the steam remains superheated. This point remains to be verified by experiment before becoming more than a hypothesis rendered probable by the known properties of steam and iron.

So far we have neglected both clearance and compression while engaged in submitting our assumed conditions to the test of comparison with actual practice, in the best examples we could find, both of condensing and non-condensing engines, with superheated, dry saturated, and wet steam.

It will be noted that a steam jacket was present in each case, and that the steam was not embarrassed by the presence of sluggish valve movements or of throttling to any considerable extent.

Indeed, it is not worth while to attempt discussion of the ill-designed and ill-conditioned engines which serve only to make the wheels of the machinery go round. The certainty of movement resulting from extreme simplicity may be safely taken as an equivalent in peace of mind, which will compensate their owners for loss in pocket, so long as the loss is the owners' and not ours.

If we substitute $\left(e - b\,\frac{B}{P_b}\right)$ for e in equation (11) and differentiate for a maximum with respect to e, we have

$$e = \frac{B\left[1 - b\,\left(1 - \text{nat. log.}\,\frac{b}{k}\right)\right]}{P_b} + k\Big] + \left\{\frac{\frac{2D}{s} - \frac{Bb}{P_b}\left[A + \frac{4D}{d}\right]}{A + \frac{4D}{d} + \frac{Bb}{P_b}\,\frac{2D}{se^2}}\right\} \text{nat. log.}\,\frac{1}{e} \qquad (16)$$

Particular attention need not be paid to the clearance and compression, except under unusual circumstances. Equation (15) must then be used to obtain a first value, which can be substituted in the second member of (16) one or more times, in proportion to the accuracy required.

Compound engines lay claim to greater economy of steam than simple engines, and the fact is undeniable that compound engines have produced economical results much more certainly than simple engines. The advocates of simple engines urge with perfect justice that com-

pound engines always have higher pressures of steam, more expansion, relatively lighter loads and more careful construction for steam economy than simple engines.

If, now, we assume the case of compounded cylinders, with cranks together or 180 degrees apart, no receiver, no drop in the expansion, no cut-off on the condensing cylinder, and no clearances or compression, we have (see Economy of Compound Engines, this JOURNAL, Jan., 1884), from addition of the equations for the work of the two cylinders,

$$PV = V_n \left\{ eP_b \left[1 + \text{nat. log.} \frac{R}{e} \right] - RB \right\} \tag{17}$$

or in terms of the volume of the condensing cylinder and of the ultimate expansion, since $\frac{R}{e} = e$

$$PV = V_c \left\{ \frac{P_b}{E} \left[1 + \text{nat. log.} E \right] - B \right\} \tag{18}$$

That is to say, mathematically it can be shown, the same measure of expansion being used in both cases, the power of the condensing cylinder alone is equal to the combined powers of the two cylinders of a compound engine, and if we neglect initial condensation the steam economy is the same. Let us see if the economy is the same.

Substituting (17) in equation (11), and differentiating, we have, for the cheapest cut-off for compound cylinders,

$$e = \frac{RB}{P_b} + \frac{2Dd}{s\,[Ad + 4D]} \text{ nat. log.} \frac{R}{e} \tag{19}$$

and substituting (18) in equation (11), and differentiating for a maximum, we would have, for a single cylinder,

$$\frac{1}{E_1} = \frac{B}{P_b} + \frac{2D_1 d_1}{s\,[Ad_1 + 4D_1]} \text{ nat. log. } E_1 \tag{20}$$

We have carefully followed the action of the steam while passing through one cylinder. Let us follow it through the two cylinders.

The steam entering the non-condensing cylinder suffers initial condensation, in some instances quite copious, but not so great as if the non-condensing cylinder had the temperature of the exhaust, T_e; however, the cut-off being later than would occur with the same ultimate expansion, E, in a single cylinder, the initial condensation will be found to be very considerable.

The steam being cut off in the non-condensing cylinder, re-evaporation begins, the expansion line being held closely to an equilateral hyperbola.

This re-evaporation is, however, far from being complete, and at the end of the stroke communication is opened to the condensing cylinder. At this instant a relatively enormous initial condensation occurs, because of the great surface of condensing piston and cylinder head presented at the temperature of exhaust, but this condensation is met at once by the equally as active re-evaporation which simultaneously occurs from the whole interior of the non-condensing cylinder, the result being the transferring of the condensation from the surface of the non-condensing cylinder to the surface of the condensing cylinder until the temperatures are equalized.

After the violence of this first transfer of condensation has abated, the re-evaporation from the interior of both cylinders occurs with sufficient alacrity to hold the expansion curve closely to an equilateral hyperbola.

If but two cylinders are used, the condensing cylinder is now opened to exhaust and the re-evaporated and vaporous steam enter the condenser, carrying much more heat than would appear from a calculation of the thermal value of the vaporous steam present at the end of expansion.

Thus we see that at the present day the compound engine owes its possible greater efficiency to the physical attributes of iron rather than to the properties of steam, and that with the use of non-conducting materials the necessity of compounding cylinders will vanish.

It does not seem as if the possible use of non-conducting surfaces for the cylinders and piston heads would greatly improve the state of affairs, since then the barrel of the cylinder, assumed of iron, would act with greater energy and consequently probably diminish the pressures much more rapidly than is now shown to be the case by the indicator diagram. This hypothesis will have to be subjected to the test of experiment before carrying any weight.

When cranks of compounded cylinders are placed at right angles and the steam is cut off from the condensing cylinder at an early point in the stroke, certainly earlier than $\frac{1}{2}$ stroke, it will be found that a considerable increase in the economy will occur with a small receiver, although this arrangement will cause trouble in equalizing the power

of the cylinders because of the increase of back pressure in the non-condensing cylinder until its piston reaches mid-stroke.

This economy arises from the fact that the transferrence of condensation from the non-condensing cylinder to the condensing cylinder is greatly facilitated by an increased difference in temperatures of the non-condensing cylinder and receiver and of the condensing cylinder.

The particularly injurious effect of a double admission of steam to the condensing cylinder, when cranks are at right angles and the cut-off of the condensing cylinder is later than one-half stroke, arises from the fact that this re-evaporation from the iron surfaces is temporarily stopped by the entrance of steam of a higher temperature from the non-condensing cylinder.

If now, in equation (19), we substitute $\frac{R}{E}$ for e, we have for compounded cylinders

$$\frac{1}{E} = \frac{B}{P_b} + \frac{2Dd}{Rs\,[Ad + 4D]} \text{ nat. log. } E \qquad (21)$$

Equation (21 places the point of cut-off theoretically for compound engines which gives the least cost, under the assumptions made, and the point of actual cut-off in the non-condensing cylinder can be determined from $e = \frac{R}{E}$ or equation (19).

The further condition that the power of the two cylinders shall be equalized, results in the equation of condition.

$$\text{Nat. log. } E = \frac{2R}{R-1} \text{ nat. log. } R - \left[1 + E\frac{B}{P_b}\right] \qquad (22)$$

or, in common logarithms,

$$\log. E = \frac{2R}{R-1} \log. R - \frac{\left[1 + E\frac{B}{P_b}\right]}{2{\cdot}3026} \qquad (23)$$

Approximate values of R, E and e under these conditions will be found tabulated in "Economy of Compound Engines" [continued from January, 1884, in the April number of this JOURNAL], the term $E\frac{B}{P_b}$ being neglected as being very small, as it usually is.

In the compounded cylinders assumed there is no cut-off on the

condensing cylinder and, therefore, T_e corresponds in equation (14) to the pressure $\frac{P_b}{E}$.

Let us make the following assumptions and compare the results:
Initial pressure abs. $= P_b = 100$ lbs. per square inch.
Back pressure abs. $= B = 2$ lbs. per square inch.
Length of stroke $= s = 4$ feet.
Diameter of condensing cylinder $= d_1 = 4$ feet.
Ratio of cylinders $= R = 2\frac{1}{2}$.

Diameter of non-condensing cylinder $= \frac{d_1}{\sqrt{R}} = d = 2{\cdot}53$ feet.

Temperature of initial steam $= T_b = 327{\cdot}57°$ (dry saturated).
Temperature of exhaust steam $= T_e = 126{\cdot}27$.
Specific vol. initial steam $= S = 267{\cdot}9$.
No. of strokes $= N = 150$ per minute.

$D = \frac{T_b - T_t}{100N}$ in which T_t denotes the temperature at terminal pressure of non-condensing cylinder.

$$D_1 = \frac{T_b - T_e}{100N} = {\cdot}01342.$$

$$A = \frac{62{\cdot}5}{S} = {\cdot}2333.$$

If now we substitute these values in equation (20), we have for the point of cut-off in a single cylinder

$$\frac{1}{E_1} = \frac{2}{100} + \frac{2 \times {\cdot}01342 \times 4 \times 2{\cdot}3026}{4[{\cdot}2333 \times 4 + 4 \times {\cdot}01342]} \text{ com. log. } E_1$$

$$\frac{1}{E_1} = {\cdot}02 + {\cdot}0625 \text{ com. log. } E_1$$

Assume $E_1 = 11$ or $\frac{1}{E_1} = {\cdot}09$, we have

$${\cdot}09 = {\cdot}02 + {\cdot}0625 \times 1{\cdot}041 = {\cdot}083.$$

That is to say, the point of cut-off will be found to be between $\frac{1}{11}$ and $\frac{1}{12}$ of the volume of the cylinder. Other considerations might in actual practice prevent so great expansion.

We can next take up the compounded cylinders. Assuming some-

thing over 12 expansions as probable in this case, let us take the terminal pressure of the condensing cylinder as 7 lbs., we have $T_t = 176{\cdot}91$

$$D = \frac{T_b - T_t}{15{,}000} = \frac{150{\cdot}66}{15{,}000} = {\cdot}0100$$

Substituting these values in equation (21) we have

$$\frac{1}{E} = \frac{2}{100} + \frac{2 \times {\cdot}01 \times 2{\cdot}53 \times 2{\cdot}303}{2\tfrac{1}{2} \times 4\,[{\cdot}2333 \times 2{\cdot}53 + 4 \times {\cdot}01]} \text{ com. log. } E$$

$$\frac{1}{E} = {\cdot}02 + {\cdot}02 \text{ com. log. } E\,.$$

If we assume $E = 18$, we have

$${\cdot}055 = {\cdot}02 + {\cdot}025 = {\cdot}045$$

or, assuming $E = 20$, and neglecting T_t we have

$${\cdot}05 = {\cdot}02 + {\cdot}026 = {\cdot}046$$

and so on until we reach an equality of the two members of the equation of condition, but it has been shown in this JOURNAL, Dec., 1883, that, all condensation being neglected, the saving by expansions surpassing 8 or 9 times is so small as not to render it worth while, from a financial point of view, to go much beyond that amount had we cylinders which were perfect non-conductors of heat. In fact we have not data justifying us in assuming that the re-evaporation from the non-condensing cylinder is efficient to any very great extent in keeping the expansion curve close to an equilateral hyperbola.

It would seem, however, that compounded cylinders do have the advantage of enabling greater expansions of steam and a greater actual economy, but they owe this advantage to the physical properties of iron not of steam.

A steam jacket in which live steam with the water thoroughly drained has been assumed for every case, as has also that sufficient superheating to insure dry steam in the steam chest. It would appear as if superheating to any greater extent remained a point to be proved of value should we adhere to iron cylinders. As compared with the total heat of steam, the amount of heat which can be added by superheating is very trifling unless we superheat to a dangerous extent.

If we assume a given number of expansions, E, which we have found *adapted to the size of engine proposed*, we can compare the

economy of a compound engine and of a simple engine with a cylinder of equal size with the condensing cylinder of the compound engine.

The work put upon the engines should be the same in both cases.

Formula (12) can then be used in each case and the resulting values of y compared.

It should be borne in mind that the proportions of the parts of an engine are functions of the initial pressures while the economy is not.

This would raise the frictional resistance of an engine with its size and with the increase of initial pressure.

The writer is not disposed to insist upon the accuracy of his present theory of condensation in more than a general way.

It has shown, however, that the wide differences in experimental results of tests of different types and sizes of engines are not irreconcileable, and that the builder of small engines of the non-condensing type is quite as right in adopting 4 expansions as the builder of enormous marine engines of the compound type is in adopting expansions of 10 or more volumes.

The data upon which the present theory is founded are all subject to revision, as they can lay no claim to scientific accuracy.

The question as to the influence of the time of exposure cannot be regarded as settled by any means and cases in which the number of strokes varies greatly from 150 may not be properly met by his formulæ.

The influence of the difference of temperature may well be subjected to thorough scrutiny in the case of superheated steam.

The effect of superheating upon the specific volume has been reduced to law by the labors of Hirn and Zenner [a most excellent resumé of their labors may be found in "Roentgen's Thermo-dynamics," translated by DuBois, published by J. Wiley & Sons, New York] but it remains to be proved that the specific volume of the steam existing in the cylinder as vapor at the point of cut-off does or does not differ from that of saturated steam.

The possible leakage of the Harris Corliss engine is far from being disproved. Repeated inquiries have failed to obtain an expression of opinion from Mr. Hill, whose language is not clear or consistent on this point of his report of 1880.

Page 4. "All the engines were new and leaked slightly through the valves and possibly in one instance past the piston during the trials. Mr. Ellis, of the Harris engine, attempted to hasten the seating of the

steam valves of his engine by filing previous to the trials with good results, as shown by the diagrams."

"No effort was made with either the Reynolds or Wheelock engine to seat the valve except by wear."

Page 6. "The trials were made in accordance with the following code of regulations, excepting (possibly to the credit of two of the contesting engines) no test was made of the tightness of the pistons."

Page 74. "The percentage of steam accounted for is very low with all the engines and is attributable largely to the natural leaks of new engines comparatively unworked. These engines would have shown better economy after a continuous use with a fair load for a period of two or three months, but with the natural leaks through valves and past pistons and a steam pressure higher than usual, it is not surprising that the engines accounted for a low percentage of the steam actually delivered to the cylinder."

It is of interest to compare this last statement of Mr. Hill with the language of G. A. Hirn, *Theorié Méchanique de la Chaleur*, 1876, Vol. II, page 55.

"In comparing the actual cost of our engines per stroke with the theoretic cost obtained by multiplying the volume opened to the steam by the density of this vapor supposed superheated, I always found the first result very much higher than the second. For a long time I continued to believe that this increase of cost arose from piston leaks. These leaks seemed to me both natural and probable, since we were working (as I then believed) at a very high temperature, capable of destroying or evaporating the lubricants used in the cylinder to diminish the friction and wear of the parts.

"I yield to M. Léloutre the whole merit of having opposed these views, of having suggested to me as both possible and probable the existence of condensation of steam during admission resembling a loss by leakage, and giving to the steam a density much greater than would be obtained by calculation. I yield to him the merit of having in a manner forced me to make new experiments which would by various methods bring the truth to light."

The writer is forced by Mr. Hill's silence and ambiguity on the point of leakage to use his own judgment in selecting a set of experiments for the calculation of the value of C (the weight of steam condensed by 1 square foot of cylinder surface heated one degree Fahr. in

one minute.) This should be an average of many experiments on different engines.

The method of treatment used is only one of many which have been relinquished because of giving impossible results when laboriously elaborated from actual data more or less deficient on important points.

So far as he knows, the writer's method is original, and has been adopted mainly for the purpose of rendering the results intelligible to those who are actually engaged in the designing and care of engines.

He would earnestly request the assistance of those in position to furnish experimental data, *to be used for scientific purposes only*, and will be pleased to send a memorandum of the methods of experimentation, and of the data required for this purpose, to those who may desire it.

His own movements are too limited, and his time too fully occupied by his duties as a pedagogue, to allow him the hope of completing the experiments necessary to a complete test of the theory put forth.

Philadelphia, January 12, 1884.

Since writing the above we have received a letter from Mr. Hill from which we quote as follows:

CINCINNATI, *February* 2, 1884.

DEAR SIR :—The calorimeter used in the tests for quality of steam at the Miller's Exhibition was of the continuous kind, carefully made and in charge of my principal assistant.

I accepted his notes and data as correct at the time, but from more recent experiments am inclined to doubt any result from a calorimeter of this kind, owing to the fact of large variations in the temperature of overflow (known to subsist by other circumstances) not recorded by the ordinary mercurial thermometer.

I now use a simple arrangement of tub, scale and hand thermometer, and while this method is liable to error, the error is not sufficient to lead you astray.

For the purpose of comparison I regard the calorimeter data of the Miller's Exhibition trials as correct, but cannot endorse it for absolute results.

* * * * * * * * * * *

I know the Harris piston was tight from special test.

Page 74. There is no doubt all the engines suffered from leaks into and out of cylinder through steam and exhaust valves.

* * * * * * * * * * *

I shall always be happy to give you any information in my power, but

owing to a busy professional life, may not be as prompt as you desire in replying to inquiries. Very sincerely yours,

JOHN W. HILL.

Mr. Hill's statements from his actual knowledge happily corroborate our own inferences from his data and diagrams, and therefore add weight to the results.

Philadelphia, February 4th, 1884.

AN INVESTIGATION LOCATING THE STRONGEST OF THE BRONZES.

By W. Ernest H. Jobbins, M. E.

(Concluded from page 199.)

Part V.

Investigations of the Author.

The work of the writer has been confined to tests by Prof. Thurston's Autographic Recording Testing Machine, and from the results thus obtained the desired values have been deduced by means of different formulæ. These results are far more accurate and reliable than could be obtained, so far as the writer is aware, by any other means; and it was possible to carry on the work with much greater rapidity. Owing to the short period of time at our disposal, it was impossible to make as exhaustive an investigation as was desired; but it is hoped that the results given will be found to be valuable. To a very slight extent these results may be considered approximate, as it was not possible to make chemical analyses of the test-pieces; but the work has been conducted with such great care and the alloys have been fluxed so carefully, in every case, with phosphorus, that the loss in casting has been reduced to a minimum.

The area chosen as the field of this investigation was a small triangular portion surrounding the peak of the mountain, Fig. 4, Part III, marked 65,000 on Fig. 3, Part III, since it was found, after a close study of Figs. 3 and 4, that this area embraced all that portion of the field in which the most tenacious alloys had hitherto been discovered. The data obtained from this investigation prove the correctness of this decision; for, though there was found a wide range in the values

obtained for the different specimens, yet they all gave exceedingly high figures, the lowest average value of tenacity being 51,139. As this research extended over but a very limited area, it has been possible to conduct the investigation with a much greater degree of exactness than has been attempted before; and subsequent investigations, covering a still smaller field, should settle beyond dispute the composition of the "Strongest of the Bronzes." Practically this research accomplishes that result.

The metals varied by differences of but one per cent. in each case, and 23 combinations were chosen; two test-pieces were made of each composition, making forty-six test-pieces in all. In the majority of cases, the data obtained from the two specimens of the same composition agreed so closely that the average value was safely made use of; but, when there was any very marked difference, the data agreeing more closely with the results anticipated from analogy were adopted, and the other value was rejected as being probably erroneous Had there been time for a chemical analysis, each specimen would have been treated separately whenever the analysis revealed any considerable difference of composition. The metals employed were of the very best commercial quality; it has been the intention throughout to obtain the maximum practical results. The copper employed was Lake Superior; the zinc, Bergen Port. Both of these metals were obtained from the Ansonia Brass Company.

In the use of tin a new departure was made; it is stated that "phosphorus is used in insuring soundness in the better class of copper-tin and copper-zinc alloys, which metals are very liable to be made seriously defective by the absorption of oxygen and the formation of oxide." After years of experiment it has been found possible to produce, on a large scale, an alloy of phosphorus and tin, which, while containing a maximum percentage of phosphorus, does not give off phosphorus when remelted. The best proportions for practical use are tin 95 per cent. and phosphorus 5 per cent. It was decided to use phosphor-tin, and the amount required was obtained from Messrs. Alfred Barber & Co., of Hamburg.

The first step was to determine the exact limits of the field selected. This was quite a difficult matter, but after careful study the following were decided upon: Copper, maximum 60, minimum 50; Zn, 48 and 38; Sn, 5 and 0. It will be noticed that these limits include the "Tobin Alloy," and also that one which Prof. Thurston mentions in

his paper as being "the best alloy for purposes demanding toughness as well as strength." Its tenacity is 68,900 pounds per square inch (4,841 kilogrammes per square centimetre). A very appropriate name for this alloy would be the "Thurston Alloy," but not his "maximum alloy," which it is here sought to identify, and it will be so designated in this paper. It was not practicable to include all the combinations possible within these limits, but twenty-three were chosen; their compositions are given in the following table :

TABLE I.

Proportions of Selected Alloys.

No.	Cu.	Zn.	Sn.	No.	Cu.	Zn.	Sn.	No.	Cu.	Zn.	Sn.
1	55	43	2	9	53	43	4	17	58	40	2
2	54	44	2	10	55	41	4	18	54	45	1
3	54	43	3	11	57	41	2	19	53	44	3
4	55	42	3	12	57	43	0	20	54	42	4
5	56	42	2	13	55	45	0	21	56	41	3
6	56	43	1	14	52	46	2	22	57	42	1
7	55	44	1	15	52	43	5	23	58	41	1
8	53	45	2	16	55	40	5				

The exact proportion of each alloy having been determined upon, the metals were weighed on the standard United States balance in the Physical Laboratory of the Stevens Institute of Technology, as described in Part IV. These weighings were made with the greatest care, as but small quantities of the various metals were used. The total weight of each test-piece was 1·45 pounds (657·70984 grams). The work of casting the test-pieces was performed in the brass foundry of the Institute; the castings were made in an iron mould, 8 inches long and 1 square inch cross-section. A mixture of plumbago and water was employed as a "blacking" for the interior of the mould, in order to reduce the friction between the sides of the mould and the cast metal to a minimum, and to prevent the test-pieces from adhering to the sides of the mould.

The copper was first placed in the crucible, and was then covered with powdered charcoal to prevent contact with the air and oxidation of the molten metal. After the copper was melted its temperature was raised, so that when the zinc was added and the consequent reduction

of temperature occurred it might not be sufficient to produce partial solidification of the metal; when this chilling occurred it became necessary to remelt the alloy, and a considerable loss of zinc was thus occasioned. Great care was taken in introducing the zinc, the metal being carefully wrapped in paper, which permitted its introduction beneath the surface of the molten copper before it could be acted upon by heat or by free oxygen; in this way a considerable loss of zinc was prevented.

The molten metals were thoroughly stirred, and the crucible removed from the furnace, after which the tin was added, the same precaution being observed as with the zinc; after another thorough stirring the metal was poured into the iron mould, which had previously been heated. The casting was then cooled as rapidly as possible. The pieces were marked as they were cast. There being two pieces of each number, they were marked, respectively, A and B. The castings were then made into test-pieces of the form and dimensions described in Part IV. Though there was a great difference as regards the brittleness of these pieces, yet none of them were so hard or brittle as to be unfitted for practical purposes, and they were all turned up in the lathe with comparative ease.

The writer next examined and discussed the data obtained in the endeavor to arrive at some definite conclusion concerning the composition and position of the "strongest of the bronzes." All the surfaces of the fractured test-pieces were very carefully examined under a strong lens. It was found impossible to detect the slightest defect in them. The figures were deduced from the curves obtained by the Autographic Recording Testing Machine. The formulas used in this investigation are as follows: $M = Wh + f$, where w = weight necessary to deflect the pencil one inch; h = height of the curve above the base line at θ_r, the angle at rupture; f = friction, and M is the torsional moment.

In this case, $w = 96{\cdot}93$ foot-pounds, and $f = 4{\cdot}75$, h being measured on the Autographic Record of each test-piece; T is the tenacity. In order to obtain the required values of T we use the formula given by Mr. Baird,* as follows: $T = [300 - \frac{1}{3}\theta_r]\, M$, in which M is known, and θ_r is measured directly from the Autographic Record The values of M, T, θ_e and θ_r, as obtained from these data, have been tabu-

* A newly discovered Relation between Tenacity and Resistance to Torsion; R. H. Thurston. Trans. Am. Soc. C. E., July, 1871.

lated; the average values of M and T have been deduced, and are included in the following table (θ_e is the angle limit) at elastic:

TABLE II.

Strength of Copper-Tin-Zinc Alloys.

Original Mark And No.		Stress in Torsion. Foot-pounds.		Approximate. Stress in Tension. Pounds per Sq. Inch.		Angles.	
		Ultimate. M.	Average.	Ultimate. T.	Average.	θ_e	θ_r
I X I	1 A	270·208	261·065	77309·5	74805·6	1·5°	43°
	1 B	251·922		72301·6		1°	40°
O B	2 A	178·321	193·361	53946·3	56653·6	1·1°	5·05°
	2 B	208·400		59810·8		0·7°	40°
Z 3	3 A	251·922	245·929	75576·6	70778·6	1°	13·77°
	3 B	219·935		65980·5		1°	10°
J 4	4 A	243·392	250·851	73017·6	73905·1	2°	19·8°
	4 B	258·319		74912·5		2°	30·3°
F 5	5 A	268·881	266·212	75824·4	75467·1	4·6°	55°
	5 B	263·543		75109·8		2°	46°
G 6	6 A	227·689	224·151	64208·3	63700·9	2·5°	53·3°
	6 B	220·612		63193·6		2°	42·1°
K 7	7 A	286·847	268·851	80910·9	75826·0	2°	54°
	7 B	250·855		70741·1		2°	53°
R 8	8 A	194·634	189·488	58390·2	56844·8	2°	9·1°
	8 B	184·331		55299·3		2·69°	5·72°
S 9	9 A	222·853	226·725	66853·9	68017·5	1·5°	5·78°
	9 B	230·597		69179·1		1·79°	4·5°
L 10	10 A	249·014	250·948	74704·4	75284·4	2·1°	4·6°
	10 B	252·881		75864·3		2·8°	8·8°
Z 11	11 A	260·645	249·014	74289·2	69116·8	2·4°	39·8°
	11 B	237·382		63964·3		1·9°	35°
D B	12 A	227·689	234·474	61020·7	61390·5	2·3°	95·2°
	12 B	241·259		61762·3		1·6°	131·4°
M 13	13 A	227·689	217·996	64208·4	61058·3	2°	52·4°
	13 B	208·303		57908·2		1·1°	65°
U 14	14 A	163·715	170·450	49113·5	51139·5	2·3°	4·9°
	14 B	177·185		53165·5		2°	7·2°
V 15	15 A	189·886	208·788	56965·8	62636·3	2·6°	4°
	15 B	227·689		68306·7		2°	5°
N 16	16 A	225·750	239·974	67725·5	71842·2	1·6°	3·8°
	16 B	253·198		75959·4		1·6°	6·8°
A 17	17 A	227·689	238·771	63200·3	68344·3	1·4°	54°
	17 B	250·952		73488·3		1·8°	43·2°
P 18	18 A	254·829	250·737	72871·1	72186·5	1·6°	43·4°
	18 B	260·645		71501·9		1·8°	54°
T 19	19 A	231·566	214·119	69459·8	64230·6	2·2°	8°
	19 B	196·671		59001·3		1·4°	4·8°
Q B O	20 A	229·628	244·168	68888·4	73250·3	1·6°	6·4°
	20 B	258·707		77612·1		1·8°	7·2°
H B I	21 A	283·908	266·768	81381·6	75135·0	2·9°	38°
	21 B	229·628		68888·4		2·4°	8°
E B B	22 A	305·233	263·508	85770·5	73378·6	2°	56°
	22 B	221·773		60986·6		2·8°	76°
B 33	23 A	225·750	200·499	63084·3	54061·4	1·6°	63°
	23 B	175·247		45038·5		1·2°	128°

From these tabulated results, and by comparison with the general characteristics of the test-pieces, the writer has determined the position of the strongest alloys very approximately. Later investigation may,

to a very slight extent, change the position of the strongest alloy. In the diagrams, which represent that portion of the field investigated, the positions of the alloys discussed are plainly marked, the numbers corresponding to those in the table just given; the other figures are explained upon the diagram. Diagram I locates the alloys.

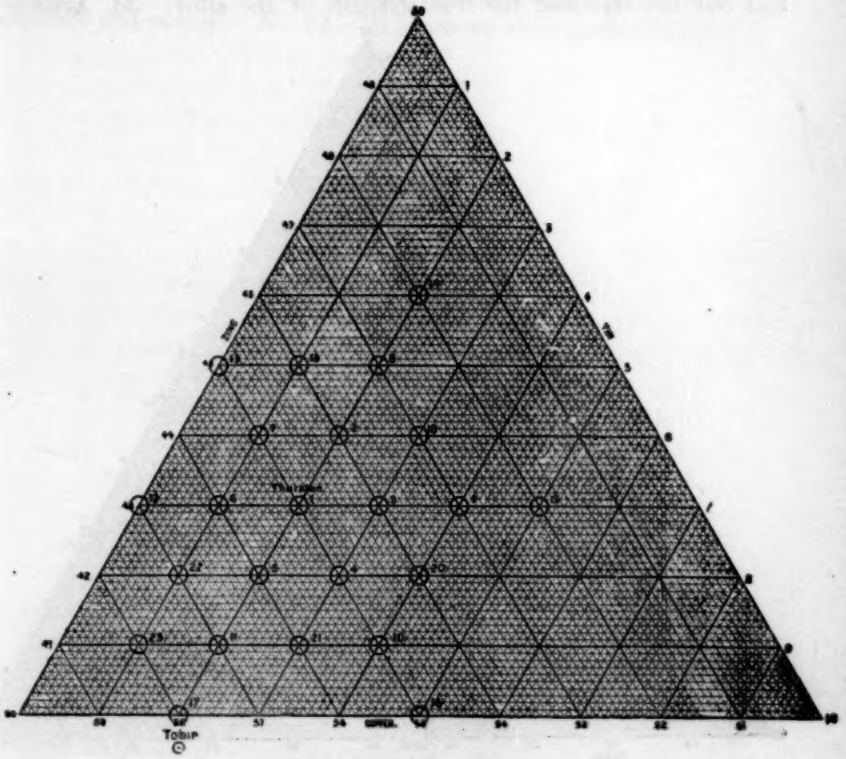


DIAGRAM I.—LOCATION OF ALLOYS TESTED.

Diagram II is based entirely upon the results obtained by the torsion tests. Diagram III exhibits the tensile strengths of the specimens. Diagram IV gives the positions of the alloys as determined by a study of II and III, and the general characteristics of the test-pieces, and may be considered practically exact. One more word of explanation in reference to diagrams II and III is necessary: In order to insure absolute certainty as to the position of the "strongest alloy" every precaution was taken to avoid error. On these two diagrams, the alloys are arranged in three different series: 1st. According to the average values

of M and T; these are marked A. This series would be correct if both castings of each specimen were exactly alike, a fact which a chemical analysis alone can settle beyond doubt. 2d. According to the highest values of M. and T, and are marked B; this series was arranged in the supposition that that the lesser values were given by poor castings, and did not represent the true strength of the alloy. 3d. After a

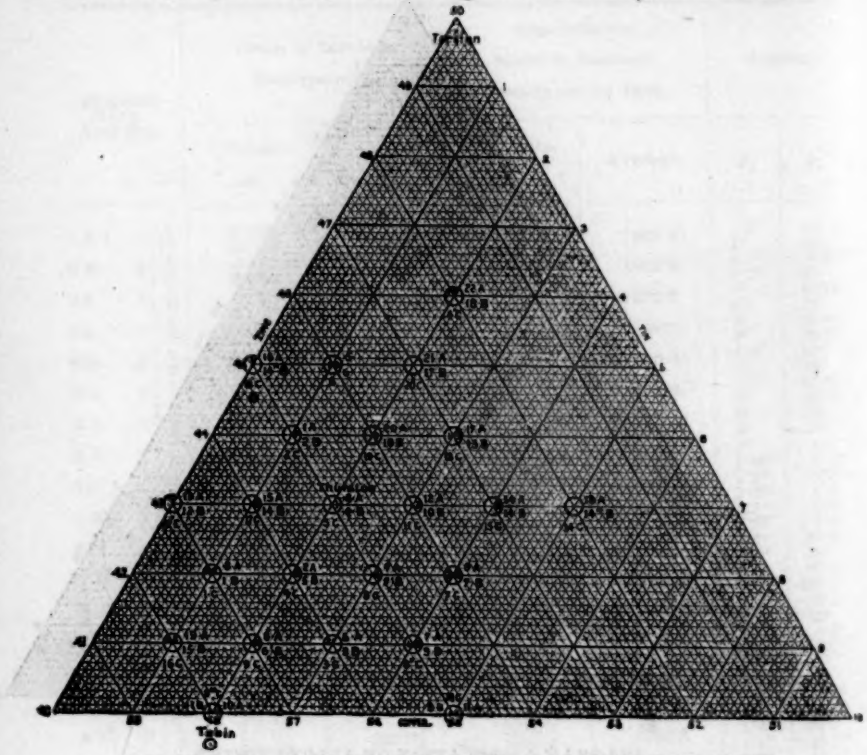


DIAGRAM II.—RESULTS OF TESTS BY AUTOGRAPHIC MACHINE.

careful scrutiny of the table, p. 264, it will be noticed that the values of M and T obtained for both specimens of the same alloy, in some cases agree very closely; when this was the case the average values were taken. In other cases the differences were very large; under such circumstances the higher values were used. From these two classes the 3d series was formed; thus it will be seen that this series is, in reality, a combination of the 1st and 2d. This series is marked C. Diagram IV was formed from a careful study of diagrams II and III. From these remarks, it will be seen that it was a matter

of some difficulty to decide satisfactorily as to the relative positions of the alloys.

The first step was to determine which alloy should be entitled to the first position; there were two rivals for this position, Nos. 7 and 22. *No. 7.*—This was a fine alloy, with a smooth, even fracture, whose surfaces presented a tough fibrous appearance and which twisted apart slowly and evenly. *No. 22.*—This was an alloy, golden in color

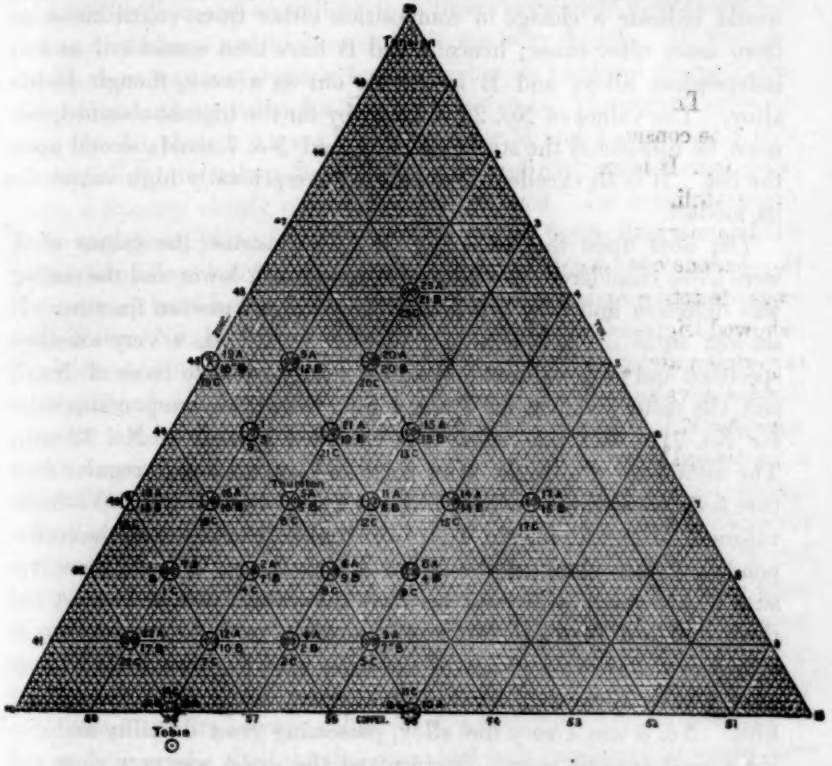


DIAGRAM III.—TENACITY OF THE "MAXIMUM" ALLOYS.

and very close grained and gave a fracture in all respects similar to No. 7, and was exceedingly tough. Thus, in appearance and general characteristics, these alloys were exceedingly alike. Consulting diagrams II and III, it was found that, when the average values of M and T were used, No. 7 stood first upon the list and No. 22 third and seventh; while, in each case, when the higher values were taken, No. 22 was first and No. 7 second and third. After a careful examination

of the values obtained for No. 7, it was thought best to consider A and B as specimens of the same alloy for the values of M and T for B, though differing considerably from those of A, are very high, and the values of θ_s and θ_r are very similar; therefore, for this alloy, the average values were taken. In the case of No. 22 there was not only a great difference in all the values of A and B, but B, while giving very low values for M and T, gave a very high figure for θ_r; this would indicate a change in composition either from volitilization or from some other cause; hence A and B have been considered as two independent alloys and B is thrown out as a weak, though ductile alloy, The values of No. 22 A being by far the highest obtained, this must be considered the strongest alloy, and No. 7 stands second upon the list. It is an excellent alloy, giving exceptionally high values for its moduli.

The next upon the list is No. 21. In this case, the values of A were alone considered, for those of B were much lower and the casting was defective and very brittle and had a ragged, uneven fracture. It showed signs of considerable liquation. "A" was a very excellent specimen and gave values of M and θ_r nearly equal to those of No. 7, and the value obtained for T was higher than the corresponding value for No. 7, in fact this value was exceeded by that of No. 22 only. The metal was of a bright straw color and had a smooth, regular fracture and considerable ductility; it was a very close third. While the values of M and T, for No. 1 A, were higher than either of the corresponding values obtained for No. 5 A and B, yet, in each, there was such a very small difference between the values obtained for A and those obtained for B, that it was impossible to do otherwise than to consider them as two pieces of the same alloy; consequently average values have been considered. This places No. 5 fourth and No. 1 fifth. No. 5 was a very fine alloy, possessing great ductility and giving a good smooth, square fracture and the grain was very close and compact. But higher results can undoubtedly be obtained from an alloy of this composition; for these specimens showed signs of slight liquation.

No. 1.—This was a tough metal, the pieces being twisted apart slowly, not snapping suddenly, as in the previous case; better results should be expected from this alloy, for it exhibited signs of an imperfect mixture of the metals. This was the strongest alloy reported by Professor Thurston to the American Society of Civil Engineers, and

described in Part III. The sixth position has been assigned to No. 11. It has been thought better to throw out all the values of B, as all the indications were, that, though a ductile specimen, it was still a very weak one and it does not fairly represent the strength of this alloy. A gave a fine regular fracture and had high ductility; it twisted apart slowly and evenly. Its values for M and T were very high. The next upon the list was No. 18. This was a good alloy, and although more crystalline than those previously mentioned, had a smooth fracture and high moduli; it was very ductile. In the case of B, there was considerable liquation; but its torsional strength was very high and its value for T a very fair one. The eighth upon the list, No. 10, though it gave high values for M and T, was still a very brittle alloy; its values for θ_r being 4·6° and 8·8° and its color gray, with a fracture closely resembling that of steel. Its tensile strength was very great, being 75,000 pounds, a higher figure than some of the preceding alloys have given; but its value for M was much lower. It was a very hard metal. No. 4 stands ninth and has even higher values for T than No. 18, but its other values were much lower. There was considerable liquation with A, while it exhibited a fracture that was smooth and regular, and it broke off slowly and evenly. In color it was a light yellow. B broke off suddenly, giving a rough granular and uneven fracture; it was of a very light gray color, indicating a brittle metal; but its values of θ_r were good, and it was quite strong. In this alloy there was 1 per cent. more zinc and 1 per cent. less tin than in No. 10, and, though yielding slightly lower values for M and T, it was far more ductile; and had there not been any liquation these values would have exceeded those obtained for No. 10. Following this alloy, quite closely, was No. 20, an alloy very bright in color, almost white, and having a ragged fracture. It was an exceedingly brittle alloy; its average value for θ_r being but 6·8°; its value of T was very good. If this value alone had been considered, it would have entitled it to a much better position; but from a consideration of all the facts it was not thought best to advance its position in the list. The eleventh was No. 16. This was a remarkably dense alloy, very hard, with a fracture closely resembling that of steel. Its values of M were quite high and those for T especially so. B was a finer specimen than A, but the difference was not great enough to warrant the rejection of A. It was very brittle. No. 3, the twelfth alloy upon the list, was slightly less brittle than the preceding, its average value

of θ_r being 11·9°. While testing A a "set" took place and this was the cause of A yielding values so much higher than B and values which are higher than can ordinarily be expected from this alloy. It broke suddenly, giving a very ragged, granular fracture; it was light in color. Thirteenth, No. 17.—This was a very ductile alloy, its values for θ_r averaging 48·6°. It was of a deep golden color, and had a smooth, regular fracture, twisting apart slowly and evenly. The composition of this alloy was very nearly the same as that of the "Tobin Alloy." Fourteenth, No. 19, was a close-grained, brittle alloy, nearly white in color, and gave a very ragged and uneven fracture; it broke suddenly. The values of A alone have been considered, the others being far too low. Fifteenth, No. 9.—This was another very brittle alloy, with a fracture closely resembling that of steel. Sixteenth, No. 6.—This was very ductile, giving a smooth, regular fracture, and breaking slowly and evenly. Its values of M and T were very fair. Seventeenth, No. 12.—This was not a triple alloy, as it contained copper and zinc only. It was an exceedingly beautiful alloy, of a very deep golden color and very closely grained. B was much the finer specimen, though there was not much difference in the values obtained. This was, by far, the most ductile alloy tested, the average of θ_r being 113·3°, though 125° would undoubtedly be a fairer average value. This was a good alloy. Eighteenth, No. 23, was the second most ductile alloy, and one of its values for θ_r was only 3.4° less than the highest values of No. 12; but the other values of this piece, B, have been rejected as being unreliable. This alloy had a fine fracture, smooth and regular. In color it very closely resembled green bronze. Nineteenth, No. 13, was also a dual alloy, and though resembling No. 12 in appearance and giving high values for θ_r, its ductility was only about one-half that of No. 12; this was expected, for the amount of zinc was increased 2 per cent. at the expense of the copper, which made it more brittle. Twentieth, No. 15, was exceeedingly brittle, and closely resembled steel in its fracture. Twenty-first, No. 2.—The results obtained for the piece B—A was thrown out—were not as expected; it was surrounded by alloys which gave much better results and therefore such a weak specimen was not looked for in this place. It was quite ductile and had a good, even fracture; it resembled No. 23 in color. Twenty-second and Twenty-third, Nos. 8 and 14.—These alloys both contained large amounts of zinc and little copper, and consequently, gave but poor results; they were both brittle and

weak. No. 14 contained more zinc than No. 8. These data have been summarized in the diagram and here it is very plainly seen what position each alloy occupies upon the field. It was concluded from the above that the peak of the mountain was at No. 22, and that the field enclosed by the dotted lines, diagram IV, contained all the best alloys. No. 6 was seemingly an exception, standing sixteenth, yet it was included because it was thought that, in a subsequent investiga-

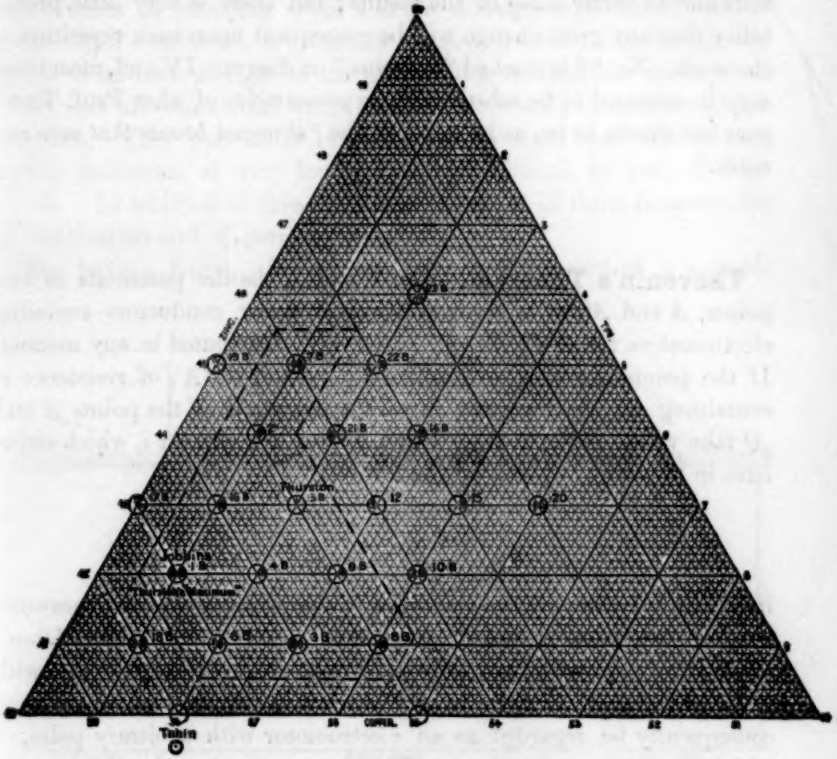


DIAGRAM IV.—LOCATION OF STRONGEST BRONZES.

tion, higher values may be obtained. It was thought doubtful whether, in another test, No. 18 would continue to hold such a high position. After a careful study of this field, the conclusion was reached that within it all is empirical. Though *No. 22* was the strongest alloy, yet, for practical purposes, it would always be better and safer to try to obtain an alloy of the composition of *No. 5*; for a slight change in the composition would not be likely to produce a

defective metal; while No. 22 stands upon an edge of the field, and a very slight variation in composition might make a very great change in strength. Therefore, in conclusion, No. 22 is Professor Thurston's "*Strongest of the Bronzes.*"* and No. 5 is the best for practical purposes. These two alloys are respectively, copper 57, zinc 42, tin 1; and copper 56, zinc 42, tin 2. (See Table I.)

It is intended at the earliest possible moment to repeat some of these tests and to verify some of the results; but there is very little probability that any great change will be consequent upon such repetition of the work. No. 22 is marked "Jobbins," on diagram IV and, meantime, may be assumed to be substantially representative of what Prof. Thurston has shown to be, as he states it, the "*strongest bronze that man can make.*"

Thevenin's Theorem.—Let V and V^1 be the potentials of two points, A and A^1, in a system of united linear conductors enclosing electromotive forces E_1, E_2, . . . E_n, distributed in any manner. If the points A and A^1 are united by a wire, ABA^1, of resistance r, containing no electromotive force, the potentials of the points A and A^1 take values different from V and V^1, but the current i, which circulates in the wire, is given by the formula

$$i = \frac{V - V^1}{r + R}$$

in which R represents the resistance of the primitive system, measured between the points A and A^1, considered as electrodes. Thus Ohm's formula is applicable, not only to simple electromotive circuits with poles well defined, but also to any network of conductors, which may consequently be regarded as an electromotor with arbitrary poles, of which the electromotive force is, in each case, equal to the difference of the potentials pre-existing at the two points which are chosen for poles.—*Comptes Rendus*, July 16, 1883. C.

---

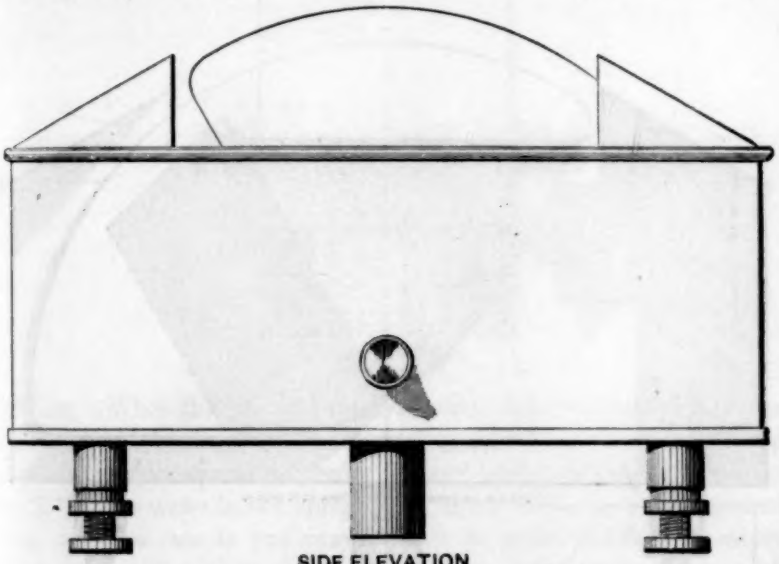
* According to Professor Thurston, this was to be looked for among compositions approximating, copper 55, zinc 43, tin 2; Strongest of the Bronzes.—*Trans. Am. Soc. C. E.*, No. 1, 1881.

A TILTING WATER METER FOR PURPOSES OF EXPERIMENT.

By J. C. Hoadley, Boston, Mass.

Having had to make, in the course of the year, a great number of experiments in pumping water filtered through sand under various prearranged conditions, in quantities ranging from little more than one gallon in an hour to eight or ten gallons in a minute, the writer soon found that all commercial water meters were wholly unsuitable for his purposes. They were liable to obstruction by sand, inaccurate at best, wildly inaccurate at very low speeds, and difficult to read at high speeds. In addition to this, the readings were at all times insusceptible of verification and of permanent record.

Not having found, after search made, any account of a suitable

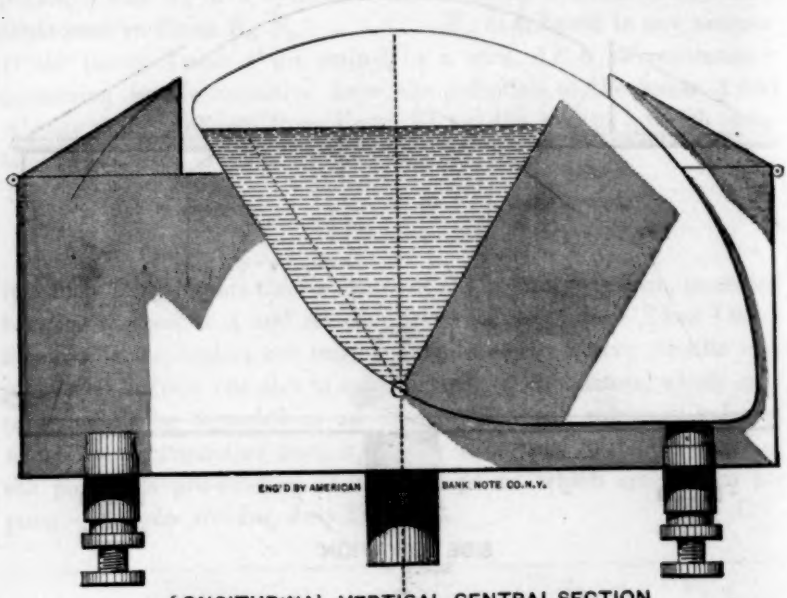


SIDE ELEVATION
SCALE ¼

instrument, it became necessary to devise one, and without presuming that this is wholly new or unknown to all the members of this society, drawings and a description of it are presented as a matter of record for what it is worth.

There was required an instrument which would measure and record with all possible accuracy, and without liability to important error, the quantity of water flowing in a continuous, but pulsating, and sometimes variable, stream, in accurately ascertained intervals of time. As constructed and used the instrument is very simple and inexpensive, and is clearly shown in the accompanying drawings.

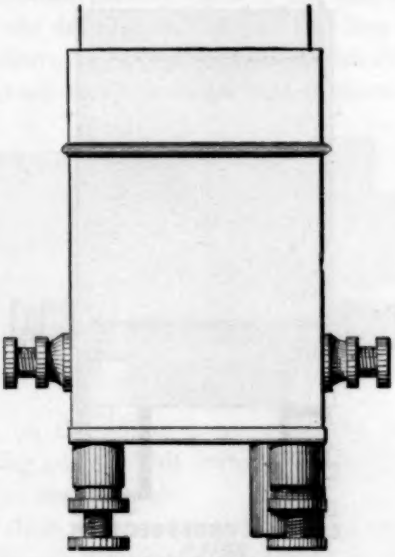
Two V-shaped cups, each embracing an angle of 60°, are joined together by a common side, which is, in fact, a mere partition between them, so that the two cups together embrace an angle of 120°. This double cup is supported in a case upon pivots directly under the partition, turning in hollow, adjustable screws in nuts attached to the case, one on each side. When one of the outside plates of the double-V cup is in a horizontal position, supported in that position by two cork stops on which it rests, the partition between the two V-cups makes an angle of 30° with the vertical, and the outside plate of the upright cup makes an angle also of 30° with the vertical, but at a little greater



LONGITUDINAL, VERTICAL, CENTRAL SECTION
SCALE ½

distance horizontally from a vertical plane passing through the axis of the pivots, on account of the curve by which the outside plate is joined

to the partition and to the outside plate of the prostrate cup at its lower end. But for this greater horizontal width on the outer side there would be no tendency to tip—the upright cup would simply fill up and overflow, and there would be an end. But as the water rises in the upright cup, the prism of water outside of a plane passing through the axis of the pivots and making an angle of 30° with the vertical, acquires constantly increasing preponderance over the equipoise of the wedge-shaped body of water bounded by planes, each making an angle of 30° with the vertical, and intersecting in the axis of the

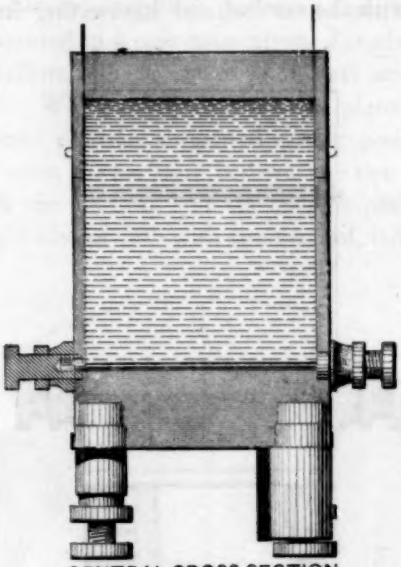


END ELEVATION
SCALE ¼

pivots. When this preponderance becomes sufficient to overcome the mechanical advantage of the prostrate cup itself over the upright cup —an advantage due to its greater leverage with equal weight—the cup will tilt, the water in the upright cup, nearly filling it, will be poured out into the case to run away through its spout, and the now empty cup, lying prone on its cork stops, will become the prostrate cup, with the greater leverage—its center of gravity being at the greater horizontal distance from the axis of the pivot. The other cup, now upright, will be filled in its turn and repeat the tilting process, and so on alternately as long as the stream flows into the cups.

The tip is very sudden, and is made with considerable force. A

light spring of sheet brass attached to the case in the middle of its length by a piece of wood which insulates it is connected by a binding post with one pole of a battery, and the case itself is in like manner connected with the other pole. A bit of sheet brass soldered to the outside of the tilting double cup, directly opposite the partition and above the case, forming a sort of cam, comes into contact with the spring in passing. This completes the circuit interrupted by the block of wood which supports the spring, and records each tip by a dot or



CENTRAL CROSS SECTION
SCALE ½

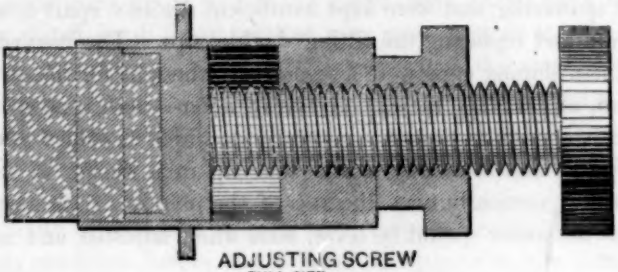
short dash. The cam does not come into contact with the spring until the tilting cup has acquired considerable momentum, so that the tilting is not sensibly retarded.

In designing this instrument the weight per square inch of the sheet tin selected for it was first ascertained. The whole of the double cup, ends, sides and partition lying on each side of a vertical plane, passing through the axis of the pivots, was then divided into simple geometrical figures, the center of gravity of each figure was found, and the horizontal distance of the common center of gravity of each portion from the vertical was computed. The weight of each portion lying on each side of the vertical was also computed, and multiplied by the corresponding horizontal distance of each center of gravity, which gave, of

course, the static moment of that portion of the double cup on each side of a vertical plane passing through the axis of the pivots.

The difference of these static moments was the preponderance of the prostrate over the upright cup—both empty—to be overcome by the water in the upright cup when full to the tilting point, and this was computed in a similar manner. A slight adjustment of the cork stops sufficed to compensate for the inaccuracy of these calculations.

This leads me to speak of the matter of adjustment by means of the cork stops. I actually used a mere socket, like a short candlestick, to hold each cork, entirely inside of the case, which was plain on the bottom. In the drawings will be seen four long sockets, projecting below as well as above the bottom of the case, with adjusting screws and check nuts. The adjustable corks are held in short sockets, with thick



ADJUSTING SCREW
FULL SIZE

bottoms to rest on the adjusting screws, heavy enough to keep the corks from floating away. This device is untried, but I think it quite practicable and no less desirable.

I had two of these meters, substantially alike, and adjusted them by flowing water through them into a tub placed on scales, counting the tips, and taking the weight and temperature of the water. I then placed one of the meters directly over the other, supported upon the tub by a suitable frame, and flowed a stream through both, the electric register recording the tips, and the scales accounting for the quantity of water. The first tip of the upper meter did not fill the lower meter to the tipping point by about $\frac{1}{16}$ inch, a little water adhering to the cup and case of the first meter, but the second tip of the first was instantly followed by the first tip of the second, and so on until the tub was filled. A repetition of this experiment gave substantially the same results—61 tips in $202\frac{1}{8}$ pounds = 3·33 pounds per tip, and at 80 F., 0·4 gallon per tip—231 cubic inches per gallon.

The electric register also recorded upon the same $\frac{1}{2}$-inch strip of

paper the beats of a seconds pendulum, and, when two pumps were used, the strokes of the pumps and the tips of each meter. It was also easy to note the tips by the ear while watching the second hand of a horse-timer, by which means the interval of time between tips could easily be observed to quarters of a second. This is not an instrument of precision. It is, perhaps, a little more accurate than a gallon measure at all practicable speeds; at slow speeds probably decidedly more accurate, and, with proper care, no considerable error is likely to occur. When operated very rapidly the swaying of the surface tends to accelerate or retard in a small degree the tipping of a cup nearly full, but there is no tendency to accumulation in such errors, which, therefore, may be presumed to balance each other, at least in some degree. The sloping covers at the ends of the case were an afterthought to prevent spattering, and were kept a sufficient distance apart to admit of taking out and replacing the tilting double cups. The curved wings under these sloping cups were a second afterthought for the same purpose, and, as it stands, the case arrests all water—there is no spattering. Of course, a suitable funnel is generally desirable to collect the water from a pump and to convey it into the cup in a stream as steady as possible, and vertically over the axis of the pivots. It is also necessary that the meter should be level, both when adjusted and when in use.

This instrument substantially weighs the water of a flowing stream, and may possibly prove useful, if strongly and delicately made, nicely adjusted and suitably porportioned to the quantity to be recorded, for keeping the record of water used during tests of steam engines and boilers. It is respectfully submitted to the profession for what it is worth.

Distillation of Benzine.—J. Chevalier, a dyer and scourer, of Toulon, employs benzine in cleaning his articles. The benzine soon becomes charged with foreign matters, is discolored, exhales a disagreeable odor, evaporates very slowly and loses its cleansing properties, so that it must be replaced by a new supply. Thinking that it might be possible to make a great saving by distilling the deteriorated benzine, he obtained remarkable results. The distillation gave a product which was limpid, much purer than the ordinary commercial benzine, ten per cent. lighter, and much more efficient in its action.—*Chron. Industr.*, April 8, 1883. C.

CAST IRON IN STEAM BOILERS.

By S. Lloyd Wiegand.

[Read at the Stated Meeting, February 20, 1884.]

Considerable interest having been expressed in the matter of the bursting of a flat boiler head, parts of which were shown at the January meeting of the Franklin Institute, and the limited supply of water having delayed the opportunity of testing the riveted iron drum with flat cast iron heads, as was intended at that meeting, all of the procurable parts of the burst boiler head are here produced, that the members may examine them.

The conditions under which the boiler head burst, have been differently stated by different parties who were present at the accident and both immediately before and thereafter, and the statements made by the same witness of the accident at different times appears to vary.

This is not an unusual occurence in regard to boiler explosions, and that it should be so will, upon a moment's consideration, appear a most probable state of affairs.

The party in charge of a burst boiler, should ordinarily know most about its condition before the accident, such persons are most apt to be killed, in which event their testimony is hopelessly beyond reach, or, if not killed and they are hurt, they are so shocked and astonished that their mental faculties are for the time impaired. But assuming that they are neither killed nor hurt, they are the parties upon whom naturally responsibility for the disaster should at first sight appear to rest and are put on the defensive to make the best showing they can for themselves, and as a rule they do the best they know how to do in that direction.

The explanations offered by boiler attendants, such under circumstances, often broadly suggest and sometimes demonstrate, both their incompetency or their ignorance.

So that after all, in investigating the causes of such disasters, the most reliable parts of the testimony are the pieces of the wreck which after being examined carefully in many cases, prove much of the oral testimony to be entirely inconsistent with demonstrated fact.

That the heads of the vessel we now propose to test may be compared with the burst boiler, which had heads made from the same

pattern, is the reason for the appearance of these parts of the wreck at this meeting.

It was alleged that the exploded boiler was under a pressure of less than 60lbs. per square inch, and that it was in fluid communication with the steam pipe of the other two boilers, which did not burst.

The steam guage of the other boilers was uninjured and proved so upon test by the maker after the accident.

The steam guage of the bursted boiler was burst and destroyed.

One of the proprietors testified, that both the safety valves of the bursted boiler sometimes stuck, and that preceding the accident several witnesses said the steam did not blow off from them.

These valves were set to a higher pressure than the engineer, and other witnesses testified was indicated by the steam guage.

The engineer testified, that there was not over 40lbs. of pressure of steam on the boiler, but that the safety valves blew off right up to the time of the explosion.

This boiler prior to being put in use, had been subjected to a hydrostatic pressure of 115lbs. per square inch.

There was much conflicting testimony taken from employés of the works where the accident occurred—the testimony before the Coroner and his jury, and the testimony given in Court, in a case in which a suit for damages was tried, resulted in a large volume of stenographic reports, which displayed more conspicuously conflicting opinions of professional experts as to the cause of the accident than anything else.

The uncontested facts that appeared were those demonstrated by the parts of the wreck—these we have here.

An endeavor having appeared to commit the Franklin Institute, by a series of ingeniously worded preambles, to a disapproval, or condemnation of flat cast iron heads, as parts of cylindric steam boilers, and the fact, that upon the docket of the United States Circuit Court for this district several cases appear analagous to, if not identical with the one already tried, it seems but proper that the members of this Institute should personally discountenance all attempts to prejudice or pervert public sentiment upon questions in litigation, and that this Institute should limit its operations, as it has heretofore consistently done, to the investigation and accurate ascertainment of exact facts in science and the mechanic arts.

The highest usefulness of this Institute having been heretofore achieved in promoting the mechanic arts and sciences (for which

purpose alone it has been chartered), by imparting reliable information and giving satisfactory reasons therefor, it is respectfully suggested that a departure from such practice and the substitution of mere opinions, unsustained by substantial and conclusive evidence and expressed either in preambles or resolutions, is both undesirable, and in defeat of the purpose of our organization and charter.

STANDARDS OF LENGTH AND THEIR SUBDIVISION.

By GEORGE M. BOND, Hartford, Conn.

[A lecture delivered before the FRANKLIN INSTITUTE, February 21, 1884.]

We are all, no doubt, familiar with the old table of English measures of length beginning "3 barleycorns make one inch." I, for one, can remember having vague ideas in regard to barleycorns in general, and their exact size in particular, though I imagined I knew exactly what constituted an inch. Later in life I began to doubt my knowledge in this respect, having had considerable difficulty in reconciling the differences between two separate inches not exactly alike, one or both evidently not $\frac{1}{36}$ part of a standard yard.

It may be of interest to glance over the history of the gradual development of the modern science of minute measurement; to notice how such crude standards as the human foot or arm, and standards called cubits, fathoms, or the foot made up of "36 barleycorns, round and dry, placed end to end," in the course of time grew into the more exact determinations of scientific research, as shown in the results of the labors of men like Kater, Baily, Bessel, Sheepshanks, Shuckburgh, and Sir George Airy in the great problem of establishing a standard of length from a natural unit. They gave us so closely the relation of the length of a pendulum beating seconds of time to the length of a yard, that it was thought they had determined, beyond further doubt, the means for restoring a lost standard should it become necessary to do so from any cause.

However good these crude standards, such as a barleycorn, a human arm or foot may have been for practical purposes at the time they were adopted, they certainly are in our times completely out of the question and useless for precise determinations. As all measures derived from them were purely arbitrary, and sanctioned by law, no reference made

to any of these sources could be presumed to restore a lost original standard, even such as a common yard-stick, except within a very liberal margin of error, we need not be surprised then to find that there happened such wide ranges of value for a foot as that of the Pythic of 9¾ inches to that of Geneva of 19 inches.

The adoption of an invariable unit as a standard of length, while seemingly only applicable to the refined methods of science, really becomes a necessity in our ordinary workshop practice, as we shall see later.

The arm of King Henry the First, or the barleycorn, though possibly furnishing a standard good enough at that time, would hardly satisfy the requirements of our modern mechanics or tool makers, who work very often within the limit of a thousandth of an inch, and even *one-tenth* of this apparently minute quantity, with surprising unconcern and no less accuracy.

To the celebrated philosopher and scientist Huyghens, is due the honor of having demonstrated the fact, that the times of the vibrations of pendulums depend entirely upon their length. About the year 1670, his inventive genius conceived the plan of using this fact to establish the length of a standard which should be the unit for measures of length. This he divided into three equal parts, each of about 13 inches, calling this third part the "horary foot."

Picard, in 1671, also proposed using the length of a pendulum beating seconds of mean time, which should be adopted as the unit of length, thus endorsing the plan of Huyghens. It was Picard who first measured the arc of the meridian from Paris to Amiens in 1669, deducing from it the value of a degree to be 68·945 miles. Picard was the first to suggest that the diurnal revolution of the earth necessarily affected the times of oscillation of a seconds pendulum, and that it ought to vibrate more rapidly at the poles than at the equator. His experiments at different latitudes, however, failed to confirm this fact, probably owing to the lack of sufficiently accurate apparatus for his work, and it was left to Richer, in the same year, 1671, to prove that at the equator, or 4° 56′ north, where the observations were made, the difference of the length of a seconds pendulum at that place, as compared with the length at Paris, or 48° 50′ north, was about a line and a quarter, or nearly one-tenth of an inch.

Cassini, in 1718 proposed a unit which should be $\frac{1}{6000}$ part of a

minute of a degree of a great circle of the earth, and which would be nearly equal to a third part of our yard.*

M. de la Condimine, who had measured a degree at the equator in Peru, in a Memoir read before the Academy of Sciences at Paris, advocated the use of a pendulum as the unit of length, proposing that it should beat seconds at the equator, a place least likely to cause prejudice that might follow from national jealousy, were the latitude of any particular place selected.

Talleyrand, in 1790, proposed to the Assembly of France that a commission be appointed to consult with a similar commission from the English Government, to consider the subject of a uniform international system of metrology. He favored the length of a pendulum as compared with the unit obtained by the subdivision of a quadrant of the earth's meridian, but after a careful consideration of the three plans proposed: the pendulum, a quarter of the equator, and a quadrant of the earth's meridian, they concluded to recommend the latter method.

In 1790, one year before the International Commission had adopted the ten-millionth part of the quadrant, as settling the question of a natural unit for a standard measurement of length, and before any steps had been taken by them in the matter, Thomas Jefferson, then Secretary of State, in obedience to a resolution of Congress calling upon the Secretary to propose a plan for establishing a uniformity in the currency, weights, and measures, for the United States, recommended, in his report, a decimal system of metrology, and that the unit be derived from a natural and invariable standard of length.

Jefferson considered that though the globe or its great circles might be invariable, the means to be employed to obtain an accurate subdivision of a quadrant, from previous trials, had showed their unreliability, and promised too great a degree of uncertainty; he also objected to the ordinary form of the pendulum as "not without its uncertainties," the length not being possible to be accurately determined, owing to variations in the clock-work mechanism and the barometric and thermometric variations. He recommended the latitude of 45° and a mean temperature of the year at that location.

Instead of using the ordinary pendulum of 39 inches, he advised the use of a seconds rod of 5 feet, known as Leslie's pendulum rod.

---

* Report on Weights and Measures, by Dr. Alfred B. Taylor, Eighth Annual Session, Pharmaceutical Association, Boston, September 15, 1859.

This was a simple straight bar, without a disc or bob, suspended at one end, and free to swing at that point, its centre of oscillation being at a distance of two-thirds of its length from the point of suspension. It would be one-half longer than the ordinary loaded pendulum.

A rod of this kind, vibrating seconds, is 58·72 inches long.

He proposed that this rod be made of iron, of such a length that at the level of the sea, at a latitude of 45°, and with a constant temperature, it should beat seconds of mean time; its length, given exactly, would be 58·72368 inches.

Jefferson then proposed dividing this length into 5 equal parts, calling each part a foot, which would give 11·7449 inches as the length of the new foot. He then divided the foot into 10 equal parts, affording a decimal subdivision to correspond with the decimal character of the coinage of the country.

The French Commission, after carefully determining the length of a quadrant of the earth's meridian, and dividing it into 10 million equal parts, presented science and the world with the meter as a universal standard to which posterity might ever afterward refer.

Its length, as they computed it, is very nearly the length of the seconds pendulum, or 39·370788 inches, or a little more than 3 inches longer than the yard.

This meter, which is an end measure standard, was made of a pure alloy of platinum and iridium, 90 parts of the former to 10 of the latter. It is called the "Mètre des Archives," and is kept in the buildings of the International Bureau, at Breteuil, between Paris and Versailles.

Having thus briefly touched upon the history of individual and national efforts to secure a unit for a standard of length, covering a period of about 200 years preceding the legal adoption of our standard yard, it may be interesting to know that just 500 years after the statute of 17th Edward II, A. D. 1324, which enacted that "three barleycorns, round and dry," make an inch, and 12 inches make one foot," it was, by act of 5th, George IV, Cap. 74 (1824), that a legal definition of the yard was made, and by it was declared that the yard-bar, made by Bird in 1760, should be the standard beyond any question or doubt.*

---

* This Act was introduced into the House of Commons in 1822, but failed to pass the House of Lords. It was again introduced, with modifications,

It may be in place to quote here an abstract of the Act of June 17, 1824, legalizing this standard, and which reads as follows:

SECTION I. Be it enacted.....that from and after the first day of May, one thousand eight hundred and twenty-five, the Straight Line or Distance between the Centres of the Two Points in the Gold Studs in the Straight Brass Rod, now in the Custody of the Clerk of the House of Commons, whereon the Words and Figures "Standard Yard 1760" are engraved, shall be and the same is hereby declared to be the Extension called a Yard; and that the same Straight Line or Distance between the Centers of the said Two Points in the said Gold Studs in the said Brass Rod, the Brass being at the temperature of Sixty-two Degrees by Fahrenheit's Thermometer, shall be and is hereby denominated the "Imperial Standard Yard." * * *

SECTION III. And whereas it is expedient that the said Standard Yard, if lost, destroyed, defaced, or otherwise injured, should be restored to the same Length by reference to some invariable natural Standard; And whereas it has been ascertained by the Commissioners appointed by His Majesty to inquire into the subject of Weights and Measures, that the Yard hereby declared to be the Imperial Standard Yard, when compared with a Pendulum vibrating Seconds of Mean Time in the Latitude of London in a Vacuum at the Level of the Sea, is in the proportion of Thirty-six Inches to Thirty-nine Inches and one thousand three hundred and ninety-three ten thousandth Parts of an Inch; Be it therefore enacted and declared, That if at any Time hereafter the said Imperial Standard Yard shall be lost or shall be in any Manner destroyed, defaced, or otherwise injured, it shall and may be restored by making a new Standard Yard, bearing the same proportion to such Pendulum as aforesaid, as the said Imperial Standard Yard bears to such Pendulum.

Just 10 years afterward, Oct. 16, 1834, occurred the calamity for which the carefully worded text of Section III was intended to provide; a contingency certainly most wisely considered. This was the

---

in 1823, but was not passed until June 17, 1824, to go into effect as stated, May 1, 1825. This was however postponed to January 1, 1826.

["Weights and Measures," by Prof. F. A. P. Barnard, Johnson's New Cyclopædia, p. 1737, Appendix. See also Encylopœdia Brittanica, 8th Edition, Vol. xxi., pp. 803 and 807.]

destruction of the Standard Yard by fire, when both houses of Parliament were burned.

The bar was recovered, but in a damaged condition, and all hopes of restoring its usefulness were abandoned, when it was found that one of the gold plugs had been melted out. The provisions of the Act now came into service, in order to reproduce the lost Standard, and it became necessary to decide whether it could be restored by the use of the method so carefully prescribed.

It had been proved conclusively since the passage of the Act that there were errors in the determination of the specific gravity of the pendulum employed; the reduction to the sea-level had been shown by Dr. Young to have been doubtful, the reduction for the weight of air was also proved erroneous, and Kater showed that sensible errors had been introduced in comparing the length of the pendulum with Shuckburgh's scale, this bar having been compared with Bird's "Standard, 1760," and found to agree closely.

Shuckburgh's scale was marked $(0 - 36^{in.})$, and was made by Troughton, in 1798, and had been compared with the pendulum and with the meter. It may be interesting to know, that as previous to Shuckburgh, all transfers of the yard were made by the use of beam compasses, and comparisons were also made in the same way.

It was not until 1798 that optical instruments were used for this purpose, and Troughton must be credited with having introduced this wonderfully improved manner of dealing with minute measurements, and which afterward, no doubt, led to the discovery of the errors found to have crept in when the relation of the yard to the length of the pendulum was established.

All attempts, therefore, to use the pendulum for the purpose of reproducing the lost standard were abandoned. The next step was to approximate this result by the use of standards then in existence, which had been compared with the original yard.

The bars used for this purpose were:

(*a*) Shuckburgh's Scale $(0 - 36^{in.})$.

(*b*) Shuckburgh's Scale, with Kater's authority.

(*c*) The Yard of the Royal Society, constructed by Kater.

(*d*) Two Iron Bars, marked A_1 and A_2, belonging to the Ordnance Department, and kept in the Office of the Trigonometrical Survey.

To Sir Francis Baily was intrusted the work of the restoration of the yard. His death occurred in 1844, before the work was completed.

He had then only completed the provisional or preliminary investigations necessary for this most important undertaking.

He had, however, made a great many experiments to determine the proper material for the new standard, and finally decided upon the alloy of which Bronze No. 1 was afterwards made. It is still known as Baily's metal. Its composition is copper 16, tin $2\frac{1}{2}$, and zinc 1.

The work was now entrusted to the Rev. R. Sheepshanks. He constructed first, a brass bar as a "working standard." This bar was compared with all the standards considered by him necessary for the purpose, and which were those just mentioned. Taking the average of all the values of each as compared with the brass bar No. 2, as the working standard was designated, and reducing to an assumed value of the original standard yard, he found as the relation of the new yard, brass bar 2 = 36·00025 inches of the lost Imperial standard, taken at 62° F. The brass tubular scale of the Astronomical Society did not appear in the list of bars used as references (see Phil. Trans. 1857, p. 661), and the statement that this was the principal authority for the new standard is therefore incorrect.

Bronze 19, as the new yard was designated, or now known as No. 1, was graduated according to this value, in terms of the lost Imperial standard, found from the comparison of these five standards, and is made, as just stated, of Baily's metal, the dimensions are: length 38 inches, depth 1 inch, width 1 inch. The graduations are upon gold plugs inserted in wells of such a depth as to bring the polished surface of the plugs at a distance from the top, one half the depth of the bar, the plugs being 36 inches apart.

The bar which is now before you is a copy in every respect, except that it has the subdivision of feet besides; but we have the same material, the same dimensions, and the same conditions in the graduations, while more than all, the distance between the two defining lines as compared with Bronze No. 1, varies less than one hundred thousandth of an inch at 62° F.

This bar was constructed by Prof. W. A. Rogers, of Harvard College Observatory, Cambridge, for the Pratt & Whitney Company of Hartford, Conn., for their use as a final reference standard. It has been compared directly with Bronze 11, at the office of the Coast Survey, by Prof. J. E. Hilgard and Prof. Rogers, and allowing for the known relation between Bronze 11 and Bronze No. 1, its value was found to be within this minute limit, in terms of the Imperial Yard.

The reason assigned for placing the lines at the centre of the depth

of the bar, was that errors arising from *flexure* were liable to occur; that is, by the bending of the bar, the distance between the lines becoming less. Having the graduations at the centre was thought would neutralize this effect. We all know that if a beam is supported at the ends and loaded in the middle, the beam is compressed at the top, and stretched or extended at the bottom, and if we were to measure between finely drawn lines, before and after the load was applied, we would find that the lines were nearer together when the beam was under strain than when free, measuring, of course, in a straight line, so it was thought that having the lines midway between the top and bottom of the standard bar, this error would be reduced.

Captain Kater was the first to discover the variations due to the flexure of standard bars upon which graduations were traced, and he first proposed a "neutral plane," which would have the effect, within certain limits, of reducing this error to zero. He first located this plane in the centre of the bar, as was done in the case of the Imperial Yard, but from further investigations he concluded that it was not quite one-third the thickness of the bar below the graduated surface.

He found that the errors from the effect of flexure depended upon the thickness of the bars compared with each other, and when resting upon a surface which is not plane (Phil. Trans. 1830). He also found that this error far exceeds that which would arise from the difference of the length of the arc and its chord under the same circumstances; so much so, that in a bar an inch thick, with the versed sine, that is the distance at the centre of the bar from the horizontal plane joining the two ends to the curved surface, equal to one hundredth of an inch, the sum of the errors would be nearly one thousandth of an inch in the length of a standard yard. To overcome the objection of a variable result at every position of a standard bar, the number of supports for it has been carefully determined, and in the case of the Imperial Bronze 1, the number of these supports is eight, and having been decided by Mr. Baily to be necessary, this was adopted for the national standards. The distance between the supports is about $4\frac{1}{2}$ inches.

Sir George Airy gave a formula for determining the distance between the supports for any standard bar, in order to neutralize the effect of flexure. It is

$$\frac{\text{Length of the bar,}}{\sqrt{n^2-1}}$$

"n" being the number of supports.

In the bar we now have before us, the conditions under which it was transferred, and also when investigated, was when resting upon *two* supports, and using the formula just given, the distance between them is about 22 inches, the total length being 38 inches. You will notice it places the supports a little less than one quarter the length of the bar measured from each end. This gives the surface a certain permanence or equilibrium of position when resting upon any *level* surface, whether a true plane or not, and if used thus under the same conditions of temperature, the distance between the defining lines remains the same. If we move the supports each nearer the ends, say an inch and a half, the surface changes slightly, and the result is to bring the lines at the end nearer together, as we have mentioned before.

According to the Report of Prof. J. E. Hilgard, Chief U. S. Coast and Geodetic Survey, in charge of Verification of Standards, in 1877, Bronze No. 1 is kept at a very uniform temperature within the walls of the Houses of Parliament, while Bronze No. 6, which is the accessible national standard, is preserved in the Strong Room of the Old Treasury, now No. 7, Old Palace Yard. There is not now any perceptible difference between these two Standards.

The Imperial Yard is in charge of Dr. Chaney, his official position being Warden of the Standards.

In order to secure, as far as possible, accurate duplicates of the new standard, four Parliamentary copies were constructed, one of which is kept in the Royal Mint, one is in charge of the Royal Society, one is preserved in the new Westminster Palace, and the other is kept at the Royal Observatory at Greenwich.

There were also 40 copies made of Baily's metal for distribution among the different governments, only two of these 40 bars are exactly standard at 62°F., these are, Bronze 19, and Bronze 28. Both are kept at the Royal Observatory for reference, as representing the national standards. All the other copies have a certain relation to Bronze 1, and instead of giving this relation, the temperature at which they are standard is established for each.

The standards prepared by Mr. Sheepshanks were legalized by Act of Parliament, June 30, 1855, and in 1856 Bronze 11, one of the 40 copies made of Baily's metal, was presented to the United States Government by the British Board of Trade, and was standard at 61·79 degrees Fahrenheit. This bar is deposited in the office of the

United States Coast Survey at Washington. It has since been found that Bronze No. 11 is *shorter* by 0·000088 of an inch at 62° F. from comparisons made by Prof. J. E. Hilgard, in charge of the Bureau of Weights and Measures, United States Coast Survey, who, in 1878, compared it directly with the Imperial Yard at the British Standard Office in London, and consequently, to be standard, must be considered so at 62·25 degrees Fahrenheit.

Previous to 1856, the distance between the 27th and 63d line of the brass scale made by Troughton, was taken as standard, though never having been legalized by Act of Congress, it had an indirect authority, as it was adopted by the Treasury Department, and copies of it were made for distribution among the different States under the charge of Mr. Joseph Saxton.

The fact is noticeable that all the copies of the Imperial Yard are made of the same material as that of the original Bronze 1. This is no doubt owing to the greater uniformity in the coefficient of expansion for each standard bar, admitting of comparisons at any temperature. This would not be possible, except for bars of other metals whose coefficient or rate of change for each degree of temperature was definitely known, and this would make it an exceedingly nice operation.

To illustrate this in a few words. If a steel bar or a platinum standard be compared with one made of brass or Baily's metal, and each were standard only at 62°, if we should compare them at 72° we would find them not alike in length, because brass expands more for each degree of rise of temperature than does the steel or the platinum; the difference would be greater in the comparison of platinum with the brass standard, as steel and brass have a coefficient more nearly alike.

Let us now briefly refer to what has been done to fix permanently the metric standard of length. The metric system is represented in Great Britain by two bars made of platinum, one being a line measure, and the other an end measure standard.

These bars are of the following dimensions:

Line Metre.....	Length	41·000	inches.
	Breadth	1·000	"
	Thickness	0·211	"
End meter..	Length	39·37 +	"
	Breadth	1·000	"
	Thickness	0·287	"

The defining lines nearly traverse the face of the bar for the line meter, and arrows, arbitrarily placed, indicate the position on the lines when measurements are to be made.

The line meter has the words "Royal Society, 45" engraved on the under side. The end meter, being made of so soft a material as platinum, is at present not in a condition to use as a standard for very accurate work, the edges of the end surface being indented and other signs of change in the surface being visible. The end meter has the words, "Mètre à Bouts" engraved on one side, and "Fortin à Paris, Royal Society, 44," on the other. These bars, together with the original standard prepared by Hassler in 1832, are the only recognized standards which have been compared directly with the "Meter of the Archives," as the French standard is called.

The Meter of the Archives is also made of platinum, the dimensions being about the same as the metric standard in London, and this bar was made a legalized standard after all attempts to make it conform to a natural unit were abandoned. It is standard only at 0° Centigrade, or 32° Fahrenheit. Thus we see, that after all, the actual use of a natural unit for creating and reproducing a standard of length was not realized; and standards, made standard by law, were really the final result.

It has been said that "a mystery is a truth hid behind some other truth, and about which the latter throws a veil," and it would seem as if this definition might apply to the great difficulties met with in the attempts to obtain a standard of length from natural laws, and natural conditions, using the grand truths which are known and accepted, but which seems to throw just enough uncertainty around the truth sought, as to make the results doubtful for the purposes for which they are intended. Truth is *exacting*, it allows for no "errors of observation" or of "personal equation," and in no other kind of investigation does this requirement seem more difficult to be fulfilled, as so many "variables"—to use a mathematical term—enter into the problem; variations of temperature, internal strain, due to position of the bar; errors of curvature, errors of observation in using optical instruments; differences in material or of density, thus affecting the rate of expansion or contraction, and a score of other variables, all tending to make the problem a complicated one. We cannot fail to realize—at least partially—the wonderful skill and patience necessary to conduct the experiments which gave us, as English speaking people, the Standard Imperial

Yard, and which 50 years ago were engaging the attention of some of the greatest minds the world has ever known.

There is still another natural unit that has been proposed as a standand of length. This is the length of a wave of monochromatic or single color light.

We have all seen the beautiful colors so wonderfully arranged in the thin film of a soap bubble. These colors are caused by what is termed "interference." To briefly explain this kind of interference, we should know that light is made up of seven distinct colored rays, which blended together produce clear white light. Each of these separate rays of color has an undalatory or wave motion through space, and the length of a wave, or the distance from the crest of one wave to the top of the next, is different for each as compared with unlike color, but constant for its own; that of the green ray, for instance, being computed as being about $\frac{1}{50,000}$ of an inch from crest to crest.

When light is reflected from the two surfaces of the thin film of a soap bubble to the eye, a portion of it must evidently travel a distance twice the thickness of the thin film of the bubble, as part is reflected from the outer and part from the inner surface of the film. The particular ray which must thus travel farther, loses a half of a wave length in the reflection, so that when these two portions of the reflected light come into the same path again, there is more or less interference, and if the retardation has been such that the wave crest of one falls into the trough of the other, they completely neutralize each other, and the corresponding color rays are destroyed. Without attempting the mathematical discussion of this subject, we know that when this relation happens more or less coincident, the rays are either deadened or are so blended that they form the beautiful rings or bands so often noticed. As the film of the bubble changes in thickness, these colors are rearranged, as different sets of color rays or waves are deadened and as different colors disappear from the reflected light.

This unit, no doubt, could be relied upon to produce a standard within certain small limits, but the addition or multiplication of such minute units for the purpose of obtaining a practical standard of length might introduce errors in the total greater than would be likely to result from either of the methods already mentioned.

The use made of this unit seems to confirm the theories in regard to the limit of divisibility of matter, and these same soap bubbles which

are such a delight to children—and we might include some of the older people too—have shown a way in which to estimate, in a purely scientific manner, the dimensions, approximately, of a molecule, a form of matter so minute that the smallest object visible under a powerful microscope is made up of countless numbers of them.

It has been demonstrated that the mechanical energy required to pull apart the molecules of water in forming steam, is no greater, according to the theory of capillary action, than is required to reduce the thickness of a film of water to the $\frac{1}{5\,000,000,000}$ of an inch; a force quite large when compared with the small amount of water which we are considering. The measurement of this minute thickness is based upon the varying colors, using the length of any given wave. Probably before this extreme tenuity could be attained, there would remain only a single layer of molecules held together by their mutual attraction, giving as the estimated average diameter of a molecule the $\frac{1}{500,000,000}$ of an inch, a dimension so infinitely minute as to be quite beyond our ability to realize.

Sir William Thomson, from a comparison of these phenomena, has estimated the limits or range of size of these minute molecules to be between $\frac{1}{250,000,000}$ and $\frac{1}{5,000,000,000}$ of an inch, and in order to give some conception of the "coarse-grainedness," as he calls it, thus indicated, he has said, "that if we conceive a sphere of water as large as a pea, magnified to the size of the earth, each molecule being magnified in the same proportion, the magnified structure would be coarser grained than a heap of small lead shot, but less coarse grained than a heap of cricket balls."

We can thus faintly begin to grasp the idea of the infinite divisibility of matter, and the science of exact measurement of length must stop far short of this limit, as it does far short of the limit of infinite extension.

We have now seen how difficult has been the work of obtaining a standard for final reference, and as it must certainly be supposed to remain an invariable or fixed length after having been once established, great care must be taken to preserve this standard from injury, caused either by wear or oxidization, or change of form.

The materials available for standards of length, taken in the order of their rate of expansion under the same conditions of temperature, are wood, glass, platinum, gold, silver, iron, brass and copper. Wood may be rejected at once for our purpose, though it does very well for

yard-sticks and pocket-rules for everyday use. Glass has been, and is now, used in certain cases, though its great brittleness makes its use restricted and the changes going on within its structure are now the subject of rigid investigation, requiring time to prove its value as a material for standards.

Platinum, alloyed with about 10 per cent. of iridium, is used for the Meter of the Archives, and also for the bars representing the line and end meter standards in Great Britain, to which reference has already been made.

Gold and silver may be said to be excluded for various reasons, that of cost in the case of gold, and its extreme softness, and silver, because of its great affinity for sulphur, which is always present in the atmosphere of cities, forming the dark sulphide that would soon ruin it for use as a standard. There is, however, a silver centimeter scale, ruled by Brunner, of Paris, subdivided into 100 parts, in the office of the Coast Survey at Washington.

Iron bars were used by the French Commission, four standards being made of this material, with polished ends. From one of them was constructed the platinum Meter of the Archives. One of these bars, the only one known to be in existence, bearing the stamp of the Commission, is now in the possession of the United States Coast Survey at Washington.

The Russian standard of length, used for geodetic surveys, was constructed of iron, using conical pieces of tempered steel in each end. This bar has a length of seven feet.

We have already noticed how largely brass, or Bailey's metal has been used for our standard yard, and for the numerous copies made of it. There remains only a brief mention of standards made of copper. M. Tresca, Acting Director of the Conservatory of Paris, constructed a copper line meter of a form which he proposed. This bar is X shaped, very light and strong, and has the lines ruled on a plane midway between the top and bottom edges.

The method adopted at the Conservatory in Paris for comparing the platinum line meter bar with the end "Mètre des Archives" is the use of a plate having the same thickness as the meter, to which is attached a thin piece of platinum terminating in a sharp point. As a statute law forbids contact of any kind whatever in the use of this platinum end meter, the reflection of this sharp point upon the surface of the end of the standard gives the means of observing

the instant of contact without contact being actually made. It is the opinon of M. Tresca that the error can be in this way reduced below 1 mikron = ·001$^{mm.}$, or about $\frac{1}{25,000}$ of an inch, in the transfer to line measure.

(To be concluded.)

ECONOMY OF COMPOUND ENGINES.

By WILLIAM DENNIS MARKS,
Whitney Professor of Dynamical Engineering, University of Pennsylvania.

(Concluded from page 48.)

It has been shown that the ratio of volume of the cylinders of a compound engine is a function of the ultimate expansion of the steam under the conditions that the power of the cylinders is equalized, and that no drop be permitted in the steam pressure during its course through the engine.

Table of ratios of cylinders and of points of cut-off in non-condensing cylinder and for ultimate expansions of steam.

$$\text{Criterion } \frac{2R \log. R}{R-1} = \log. 2{\cdot}7183\ E$$

$$e = \frac{R}{E}.$$

Ratio of cylinder volumes.	Ultimate expansion of steam.	Point of cut-off in non-condensing cylinder.
R	E	e
1¼	3·426	0·37
1½	4·190	0·35
1¾	5·015	0·34
2	5·886	0·34
2¼	6·813	0·33
2½	7·801	0·32
2¾	8·840	0·31
3	9·933	0·30
3¼	10·961	0·30
3½	12·277	0·28
3¾	13·580	0·28
4	14·832	0·27

REMARKS.

These computations are made for general guidance only, nearly all the assumptions made being impossible of exact realization.

It is assumed that a perfect gas is used expanding isothermally, that there is no back pressure on the condensing cylinder piston, and that there are no clearances or receiver, and further that there is no cut-off at all for the condensing cylinder, that only being demanded to provide for receivers and clearances in actual practice, and that the cranks are together, or 180 degrees apart.

It is useless to carry this table farther. Enough has been done to show the error of exaggeration of ratio, into which designers have fallen, when it is necessary to equalize the power of cylinders and to avoid an intermediate drop for the sake of economy.

Indeed in all engines it will be found that economy of steam as well as smoothness of action, demand that no sudden changes of pressure shall be permitted, and therefore it will be found advantageous in single cylinder engines where clearance cannot be indefinitely reduced to use enough compression to bring the back pressure up to the initial pressure, and so as not to permit an *explosion* in the cylinder at the beginning of each stroke.

The less the clearance the less the compression required for this purpose, and consequently the less the power of the engine is absorbed in fulfilling this condition.

We see, therefore, that clearance is particularly injurious where vacuums are very perfect, and every means should be used to reduce it.

On the other hand, if convenient, very considerable clearances may be used in non-condensing engines provided it is met by a proper increase in the size of the cylinders to compensate for reduction of power and a proper compression is used.

When compound engines must be frequently started or reversed it is important that they be so arranged as to avoid getting on their centres; this does not apply to pumping engines, or indeed to the majority of stationary engines, but does apply with a great deal of force to marine engines.

Some designers have endeavored to obviate this difficulty by placing the cranks 160 degrees apart, thus enabling a very small dead space between the cylinders and obviating trouble with the engine on its centre.

This case differs so little from the case already considered with the cranks together, or 180 degrees apart, that it requires but one precaution on the part of the designer.

The non-condensing cylinder should exhaust *before* the condensing cylinder takes steam, not after, as that would cause a sudden rise in the pressure of the condensing cylinder with the attendant loss.

A good deal of weight is laid on equalizing stresses by placing the cranks 90 degrees apart, but as a very large number of engines of the first type have been successfully designed, there is no reason to believe it impossible in the future to use engines with cranks 180 degrees apart.

It is quite possible to give sufficiently large clearance spaces in the case of engines with cranks at right angles to fulfill to a large extent the functions of a receiver, but it is more economical to reduce the clearances of the the steam cylinders as much as possible and to provide a receiver of proper size. Since this will avoid sudden changes in pressure and the consequent loss.

In what follows we will neglect the variation of the piston's positions due to the angular position of the connecting rods.

Assuming the engine to have obtained its regular movement we will have $P_r = \frac{eP_b}{e_1 R}$ (1)

(See JOURNAL FRANKLIN INSTITUTE, January 1884.)

The sequence of exhaust from non-condensing cylinder, and of cut off of the condensing cylinder, renders necessary the presence of a receiver or its equivalent when cranks are at right angles. The exhaust from the non-condensing cylinder occurs just as the piston of the condensing cylinder reaches mid stroke, and so in order to avoid a sudden change of pressure and of the progress of expansion in the condensing cylinder, it is necessary that its cut off be earlier than one-half stroke.

The size of the receiver only determines the fluctuation of the pressures in it. The smaller the receiver the greater the fluctuations.

It is obvious from what has already been shown that at the instant of cut off of the condensing cylinder, the pressure in it and also in the receiver, as well as the back pressure in the non-condensing cylinder equals P_r.

At the instant of reaching the end of the stroke of the non-condensing cylinder it voids into the receiver its steam at a pressure eP_b.

If we wish this event to occur with as little disturbance as possible we must make the pressure at that event equal in non-condensing cylinder and receiver.

The cranks being at right angles have certain definite positions with regard to each other at all times, and the position of one piston being fixed that of the other can be deduced. If now we assume the cut off e_1 of the condensing cylinder to be $\frac{1+k}{2}$ it will be coincident with the exhaust of the non-condensing cylinder, and the equation

$$P_r = \frac{eP_b}{e_1 R}$$

becomes $e_1 = \frac{1}{R}$ that is $R = 2$, very nearly.

The exit from the receiver to the condensing cylinder being closed, the piston of the non-condensing cylinder now presses back the steam in that cylinder and the receiver until it has reached half stroke, when the pressure is a maximum P_m, we can write the following equation

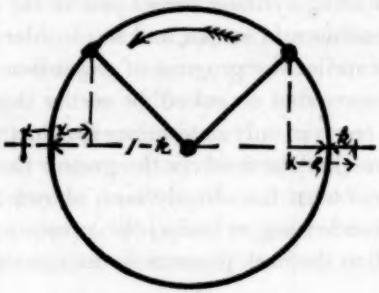
$$eP_b\left(V_n + V_r\right) = P_m\left(\frac{1+k}{2}V_n + V_r\right) \qquad (2)$$

In this equation we can fix P_m and determine V_r or vice versa.

In general we can say that e_1 must be somewhat greater than $\frac{1}{R}$ in order to receive the exhaust steam from the non-condensing cylinder without forcing it back.

If we assume that the terminal pressure of the non-condensing cylinder must equal that in the receiver at the moment of opening

FIG. 1.



communication. We can write the following equation, in which $x =$ the distance of the non-condensing piston:

$$P_r\{xV_n + V_r\} = eP_b(kV_n + V_r) \qquad (3)$$

$$x = \frac{1+k}{2} - \sqrt{e_1(1+k) - e_1^2 - k}$$

We have then

$$\frac{V_r}{V_n} = \frac{e_1 Rk - \frac{1+k}{2} + \sqrt{e_1(1+k) - e_1^2 - k}}{1 - e_1 R} = r \qquad (4)$$

At the instant of cut off a quantity of steam is left in the receiver and non-condensing cylinder at a pressure P_r. As the cut off e_1 is assumed earlier than half-stroke we can then write the following equation.

$$P_r(xV_n + V_r) + eP_bV_n = P_m\left\{ V_r + \frac{1+k}{2} V_n + kV_c \right\}$$

The two indeterminate quantities in this equation are P_m and V_r, either of which can be fixed and the other deduced. It will be observed that the clearance of the condensing cylinder is included and therefore that P_m is the pressure at the instant the piston of the condensing cylinder is at the end of the stroke its valve being assumed to have lead and a perfect vacuum to be obtained.

Neglecting the clearances in the cylinders, we have:

$$\frac{[\frac{1}{2} + \sqrt{e_1 - e_1^2}]V_n + V_r}{e_1R} + V_n = \frac{P_m}{eP_b}\left\{ V_r + \frac{V_n}{2} \right\} \tag{5}$$

or

$$\frac{[\frac{1}{2} + \sqrt{e_1 - e_1^2}] + r}{e_1R} + 1 = \frac{P_m}{eP_b}(r + \tfrac{1}{2}) \text{ therefore}$$

$$r = \frac{\dfrac{P_m}{2eP_b} - \dfrac{[\frac{1}{2} + \sqrt{e_1 - e_1^2}]}{e_1R} - 1}{\dfrac{1}{e_1R} - \dfrac{P_m}{eP_b}} \tag{6}$$

This can be compared with equation (4) clearances neglected which places the condition of no drop.

$$r = \frac{\sqrt{e_1 - e_1^2} - \frac{1}{2}}{1 - e_1R} \tag{7}$$

It will be observed that the presence of a receiver has the effect of reducing the power of the non-condensing cylinder when P_m is greater than eP_b as it always should be. The greater the size of the receiver the less the increase of P_m, but this increase of P_m being made gradually and the power taken from the non-condensing cylinder being restored to the condensing cylinder by reason of the increased pressure before cut off occurs it would not seem detrimental to economy to make receivers as small as possible.

If we assume that at end of stroke the pressures in non-condensing cylinder and receiver are the same, we have

$$eP_b[V_n + V_r] = P_m\left(\frac{V_n}{2} + V_r\right) \tag{8}$$

Letting $P_m = heP_b$ and equating the values of r from 7 and 8.

$$e_1^2\left\{\frac{R^2}{4}\left(\frac{h-2}{1-h}\right)^2+1\right\}-e_1\left\{1+2R+\frac{R}{2}\left(\frac{h-2}{1-h}\right)\right\}=$$

$$-\tfrac{1}{4}\left(\frac{h-2}{1-h}\right)^2-\tfrac{1}{4}-\tfrac{1}{2}\left(\frac{h-2}{1-h}\right) \qquad (9)$$

$$\text{Let } A=\tfrac{1}{2}\left(\frac{h-2}{1-h}\right) \quad \text{Therefore } h=\frac{2A+2}{1+2A}$$

$$e_1^2[R^2A^2+1]-e_1[1+2R+RA]=-A^2-A-\tfrac{1}{4}$$

$$e_1^2-e_1\frac{[1+2R+RA]}{R^2A^2+1}=-\frac{A_2+A+\frac{1}{4}}{R_2A_2+1}$$

$$e_1=\tfrac{1}{2}\frac{[1+2R+RA]}{R^2A^2+1}+\sqrt{\frac{[1+2R+RA]^2}{4(R^2A^2+1)^2}-\frac{A^2+A+\frac{1}{4}}{R^2A^2+1}} \qquad (10)$$

or
$$A=\frac{2\sqrt{e_1-e_1^2}-1}{1-e_1R} \qquad (11)$$

Since in this equation e_1R must always be greater than unity the denominator must always be a minus quantity.

$$\text{Let } e_1=\cdot45 \quad A=\frac{2\sqrt{\cdot45-\cdot2025}-1}{1-1\cdot125}=\frac{+\cdot994-1}{-\cdot125}=\cdot048$$

$$\text{Let } R=2\tfrac{1}{2}$$

$$h=\frac{\cdot096+2}{1+\cdot096}=1\cdot91$$

$$r=\frac{\cdot497-\cdot500}{\cdot125}=\cdot024$$

Again, let $e_1=\cdot50$, the latest point at which we can cut off.

We have $r=0$, $A=0$ and $h=2$.

We see that the conditions, no drop and great economy of steam, are directly opposed by the demand for equality of power in the two cylinders.

The disadvantage of these high back pressures at mid-stroke in the non-condensing cylinder arises from the diminution of its power.

The only result of using a very large receiver when cutting off steam earlier than the point $\frac{1}{R}$ is to prevent the pressure in the receiver from rising much higher than the terminal pressure of the non-condensing cylinder.

A high back pressure at mid stroke of the non-condensing cylinder

also means a high initial pressure of the condensing cylinder, and consequently increased power in the condensing cylinder.

That is, a large receiver operates to prevent disproportion in the power of two cylinders when proportioned according to the criterion given in the first part of this paper, requiring also that the point $e_1 = \frac{1}{R}$ very nearly, but will be found not to be so economical of steam as a very small receiver or none at all.

The clearances necessarily are regarded as receiver space.

It is very difficult to deduce the mean effective pressure upon the piston head of the condensing cylinder anterior to the cut-off.

If, however, we are satisfied with an approximation as a guide, we can assume $V_r = 0$ and no clearances, and write the following equation for the purpose of equalizing the power of the two cylinders for equal strokes:

$$P_r = \frac{e P_b}{e_1 R}$$

$$V_a \left\{ e P_b \left(1 + \text{nat. log. } \frac{1}{e}\right) - 2 P_r \text{ nat. log. } 2 \right\} =$$

$$V_c \left\{ \frac{3}{2} P_r e_1 + e_1 P_r \text{ nat. log. } \frac{1}{e_1} \right\}$$

$$\text{Nat. log. } \frac{e_1}{e} = \frac{e_1 R + 2{\cdot}772}{(2 e_1 R)}$$

$$\text{Com. log. } \frac{e_1}{e} = \frac{e_1 R + 2{\cdot}772}{4{\cdot}6 e_1 R}$$

In which we have the limitations e_1 must not be greater than $\frac{1}{2}$ and $e_1 R$ must not be less than unity.

To sum up the discussion:

With cranks at right angles, we cannot cut off later than one-half stroke in the condensing cylinder without a double admission to it, and consequent loss.

We cannot cut off earlier than $\frac{1}{R}$ without pressing the steam from the receiver back into the non-condensing cylinder, because the steam will rise to a higher pressure in the receiver than the terminal pressure in the con-condensing cylinder. If this is done only to a small extent it may not prove a serious evil.

If we use a receiver of any considerable size we must submit to a drop from the terminal pressure of the non-condensing cylinder to the pressure in the receiver, with the consequent loss, or make $e_1 = \frac{1}{R}$ very nearly.

If we do not use any receiver, or a very small one, we can effect a greater economy of steam, but the power of the two cylinders cannot be equalized without pushing the ultimate expansion beyond all reasonable limits, diminishing the concentration of power so essential to all steam engines, unless we make the point of cut-off of the condensing cylinder later than $\frac{1}{2}$ stroke, and thus submit to a double admission.

In conclusion the writer would like to say that he does not believe that a final agreement will be reached from one discussion or many discussions, or that we can refer our reasoning back to nature and prove its correctness or its superficiality, until a more complete and rational set of experiments are made on the compound engine than now exist.

A famous English lawyer is reported to have said that he could "drive a coach and four through any act of Parliament."

It would require but very little effort to do the same to any set of experiments now extant upon the compound engine.

Forms of Comets' Tails.—Th. Schwedoff has shown that comets' tails may be represented by waves, produced by the passage of the nucleus through a resisting medium. His theory has the advantage of dispensing with the assumption of any doubtful agencies, and of predicting the form and position of the tail in any comet of which the elements are known. Faye has endeavored to demonstrate that the hypothesis of an interplanetary resisting medium would require a mass, within the limits of our system, a hundred thousand times as great as that of the sun, even if the density of the medium were only $\frac{1}{2,000}$ as great as that of our atmosphere. He supposed, however, that the æthereal density does not vary between the surface of the sun and the orbit of Neptune. If we admit, with Encke, that the density of the cosmic medium decreases in inverse ratio to the square of the distance, we readily find, for the total mass of the medium, a value which is only $\frac{1}{13,000,000}$ as great as Faye assumes.—*Comptes Rendus*, May 7, 1883. C.

REPLY TO MR. THEO. D. RAND'S PAPER,

ENTITLED "NOTES ON THE GEOLOGY OF CHESTER VALLEY AND VICINITY, IN THE PROCEEDINGS ACADEMY NATURAL SCIENCES, NOVEMBER, 1883.

[PREFATORY NOTE.—In the *American Naturalist* of May, 1883, the writer reviewed a publication of the Mineralogical and Geological Section, marked "No. 2, 1880–1881." The only articles in this publication of any length or importance were by the Director, Mr. T. D. Rand.

Both of these were mainly devoted to criticizing the work of Mr. Chas. E. Hall, formerly an Assistant on the Second Geological Survey of Pennsylvania. While many of the criticisms may have been well founded, it seemed to the present writer that the tone of the criticism was unjust, and the implied charge of incompetency, unfounded. As Mr. Hall was absent from the city, and unable, therefore, to undertake his own defence, the writer replied for his former colleague, from whose views, however, he differed widely, in the *American Naturalist* of May, 1883. Mr. Rand rejoined in September of the same year, raising, among other questions, that of the accuracy of one of my dates, which was therefore established on the authority of the Secretary of the Board, Mr. Forman, in a short note in the same publication in October.

In the meantime, in the September number of the FRANKLIN INSTITUTE JOURNAL, Mr. Rand had criticised, with considerable severity, over his well-known initials, the volume C_4, which had recently appeared. This led to an answer by me in the October number of the JOURNAL.

Mr. Rand then, under the title of "Notes on Chester Valley and Vicinity," read a paper strongly combating views which had been, in various places, expressed by or ascribed to me. I had not expected this, though I was accidentally present at the meeting, and replied orally to the points that I happened to recall. Mr. Rand was kind enough to allow me to see his manuscript, in which I found some expressions which I did not remember to have heard.* My reply to this paper, written with the manuscript before me, was submitted to the Mineralogical and Geological Section for publication with Mr. Rand's paper, and was not published by that committee, but referred to the Publication Committee of the Academy itself. This committee accepted it, provided three passages marked by the committee were omitted. In the following, these passages are italicized. I considered them of trifling importance, but could not acknowledge the justice of the committee's demand, and withdrew the paper.

The second of these three passages concerned the present writer alone, and, as he did not feel too badly about it, he was at a loss to comprehend the vicarious sensibility of the committee.—P. F.]

Mr. Theodore D. Rand presented to the notice of the Mineralogical and Geological Section of the Academy of Natural Sciences, on Octo-

* I was absent for a few minutes during the reading of Mr. Rand's paper, for the purpose of getting from the library one of the volumes criticized.

ber 21, 1883, an interesting series of rocks, illustrative of the belts in the vicinity of the region where Chester, Delaware and Montgomery counties join; and has given utterance to some categorical criticisms on the newly issued volume C_4 of the publications of the Second Geological Survey of Pennsylvania.

There are eight of these numbered criticisms, but this does not include quite all, because sometimes two criticisms are included under one number; and the criticisms continue after the numeration has ceased, *as if the mathematical faculty could not keep pace with the multitude of impressions due to a sensitive geological judgment.* It is perhaps fortunate for me that, in replying to those criticisms which concern me, I am able to set aside, at the outset, so large a proportion, relating to matters for which I am in no way responsible, and which I saw for the first time after the volume C_4 was printed.

To speak more specifically, criticisms 2, 3, 4 and 7 are such as have just been alluded to. They are directed at statements made by other persons than myself, and with which I either have no sympathy or concerning which have formed no opinion.

No. 6 is really in the same category as the preceding, because it attacks simply a quotation, for which I gave the authority, regarding a state of things outside of the district I was studying, and which I had never seen.

There remain three numbered and four unnumbered criticisms. In regard to these collectively I cannot repress my surprise at their inaccuracy; and in most cases so great is the inaccuracy that were it not for Mr. Rand's (no doubt unintentional, but careless) misquotation, there would be nothing left to base the criticism upon. For example, Mr. Rand quotes as follows: "C_4 says, pp. 34, 124, 'The quartzite failed altogeter on the southern side of the valley.' 'No Potsdam sandstone has been detected anywhere along the south edge of the limestone area.'"

If the reader will turn to p. 34, he will find the sentence preceding the first just quoted to read thus: "When *Chester county* was reached, all sharply defined boundaries ceased to be possible. The quartzite failed," etc., as above.

If one turns to the bottom of p. 123, he reads the following from Prof. Lesley: "Prof. Frazer draws attention to the fact that the *Lancaster county* limestone plain does not seem to rest upon a floor of Potsdam everywhere, but only in its middle and northern parts. The

uplift which crosses that county east and west just north of the city of Lancaster, exposes the Potsdam sandstone several hundred feet thick in Chikis rock on the Susquehanna above Columbia and also at the west end of the Welsh mountain (Laurel Hill, in Earl township), and the Potsdam sandstone is exposed more or less evidently along the edge of the limestone along the Chester county line as far south as the Gap in the North Valley hill and somewhat further west, but no Potsdam sandstone has been detected anywhere along the southern edge of the limestone area, it seems to be replaced by Roger's primal slates."

It is evident from the above complete quotations that on p. 34 I was speaking of *Chester county exclusively*, and on p. 124 Prof. Lesley was citing my opinion of structure in *Lancaster county exclusively*." Inasmuch as the supposed occurrences of Potsdam near the King of Prussia, and in Cream Valley, mentioned by Mr. Rand, are in Montgomery county, which was out of my field of labor, and was not included in either of the citations, his criticism contains nothing to answer.

The second criticism refers to the words, "sandy gneiss"* and "hard serpentine-like mineral," which are supposed to occur on p. 283. What is said there is, "A decomposed friable white gneissoid rock, together with a hard serpentine-like rock, cross the railroad about Wayne Station." The former is evidently the Eurite, about which Mr. Rand has so much to say.

The second observation is very vague, as its author often permitted himself to be in regions outside of that which was the object of his study, viz., Chester county.

How close or what was the analogy between the hard rock in question and serpentine he cannot now say, nor was it his business to say anything about the locality at all. To conclude from "serpentine-like" that the author meant serpentine, would be to entirely alter the intended sense of the passage. But it is worth while to observe how very inexact this quotation is.

Criticism No. 8 seems the only one fairly justified by the text of C_4, p. 282. The context does seem to imply that the serpentine and schistose matter were together in place. With the dip mentioned and fairly within the county of Chester, I frankly confess I am unable,

* Since retracted in a foot-note.

from memory, to state to what this observation refers, as the field notes were made four years ago. I further confess that having recently examined the quarry near the Spread Eagle, and on the north side of the Lancaster turnpike, I am prepared to state that it is in a dark gneiss, as different from serpentine as it is from the white "Eurite" near Wayne, which, as well as itself, is ascribed to the Potsdam by Mr. Rand. I must bear the blame of an error here for the present.

The first unnumbered criticism of my work refers to an article published by me in the May number of the *American Naturalist*, where I am said to have contended rightly "that the region contains hydromica schist *only*." I challenge Mr. Rand to produce evidence for this statement. Certainly the rocks which preponderate are schists, containing mica of the Damourite group, and these are the rocks that have been taken for talc schists, but I have nowhere said that they were the *only* rocks exposed in this region, nor have I ever thought so. In answer to the question, "Do they contain chlorite?" I reply, I believe they do.

The second unnumbered criticism is based upon an error, but this time of Mr. Rand himself, which he handsomely acknowledges in a foot-note. In the September number of the FRANKLIN INSTITUTE JOURNAL, p. 228, he says: "The trap (pp. 87 and 218) on the north side of the serpentine, Easttown, is not on the map." To this statement I demurred. In point of fact, it may be observed there plainly. Of this Mr. Rand says, in his new criticism: "I inadvertently located this" [absence of trap] "in Easttown. It is really in Willistown." As to the new charge of omission, I will examine into it at the first convenient opportunity.

Mr. Rand desires to know if in stating that Rogers' altered primal, as colored on his map, included a heterogeneous collection of rocks, I may not have included with it some adjacent rocks, I refer him for answer to the large and complicated area covering parts of Chester and Delaware; the entire southern portion of Lancaster, from Turkey Hill to Maryland, and all that portion of York southeast of the limestone belts. This territory contains within it all the rocks I have named.

If there be any allusion to the serpentine belt of Radnor and Easttown on page 33 of the publication of the Mineral and Geological section, it is so covert that I fail to perceive it; certainly there is no "assertion" to that effect, which was Mr. Rand's original language. Page 34 being a map, naturally contains no assertion of any

kind. But, as I said before, this is mere trifling and waste of time, and was good-humoredly introduced to show how Mr. Rand's own method of criticism would apply to his own work.

That Mr. Rand described his "echelon structure," not as a theory, but as a fact, I am quite willing to believe. I did not go further than to suggest as a possible hypothesis the structure I proposed. *If subjective certainty be the test of accuracy, I grant that Mr. Rand is more likely to be right than mere students of nature.*

Finally, it is indisputable that this firing at long range in the columns of scientific journals and the Proceedings of scientific bodies (in regard to phenomena almost at our doors) is neither profitable nor edifying. I thank Mr. Rand heartily for every error he has exposed, whether mine or another's, and my only wish is that the truth may become apparent at the earliest moment, no matter whose theories may be overthrown.

In the above criticisms, out of thirteen, numbered and unnumbered, but seven apply to my part of C_4. Of these seven, but one is well founded, one is trivial, and the other five are based upon such inaccurate statements, that to four of them, were it not for Mr. Rand's high character and conscientious work I would not reply at all. Under these circumstances Mr. Rand should practice a large charity for the shortcomings of others. I quite believe that his observations were made on foot, and I make no reply to the ungenerous charge that "if all the observations of *** C_4 had been thus made there would have been fewer blunders to correct," except to say that the above examples are very far from proving this statement. That there are many and serious errors in my part of C_4 is probably true: (how could it be otherwise with notes printed without the supervision of their author a year or two before they were intended for the press); but Mr. Rand has only brought possibly one of them to light.

PERSIFOR FRAZER.

In reply to Dr. Frazer, I would say:

1. C_4 and my paper are published. The accuracy of my quotations may be verified by any one who desires.

2. I submit that the observer going southeastwardly from Lancaster county (C_6 p. 34), the phrases, "When Chester county was reached * * * the quartzite failed altogether on the southern side of *the valley*," and (p. 281) under Tredyffrin. "This is the easternmost of the valley tier.* * * *

*Hence next to Montgomery and within a very short distance of the sandstone outcrops described.

Absence of the quartzite on the southern edge of *the valley*, a problem of the utmost difficulty," naturally import the *whole Chester Valley*, and not a part of it only, and the contention that on p. 124, Prof. Lesley was speaking of Lancaster county exclusively, would have fallen had the quotation been continued but a few lines further, as follows: "It is not unreasonable therefore to suppose that the original *southern limit* of deposition of the sandstone was a line drawn from Pomeroy west to Columbia.* * * But this renders it the more extraordinary to find exposures of what seems to be Potsdam sandstone * * * in *southern Chester county*."

If it is admitted that a sandstone does occur on the southern side of the Chester valley, very close to the Chester county line, as described by Prof. Lewis, in 1879,* it would seem that the county line should not have been an impassable barrier to its examination, as prominence is given to the supposed absence of sandstone in that position. In other parts of the work there are observations in Montgomery and Delaware counties.

3. Is it seriously contended that unaltered diorite is a "serpentine like" rock?

4. As to chlorite in the hydromica schists, I quote from Dr. Frazer's criticism of C_4 in the *American Naturalist*, October, 1883. "These rocks contained no talc whatever, the mineral that was taken for talc being a hydrous mica of the damourite group; * * * yet the old name, talc mica region is retained as if it actually defined something, * * * the magnesia which these schists are supposed to contain (contrary to the results of repeated analyses by Dr. T. S. Hunt, Dr. Genth, Mr. McCreath, the writer, and many others), is made the basis of an hypothetical speculation."

From this I inferred, it seems wrongly, that Dr. Frazer contended that these schists were exclusively hydromica schists. I regret that I misunderstood him, but I cannot yet see how schists, which contain no magnesia, can contain chlorite any more than they can contain talc. Can he refer to an analysis of specimens from a locality not close to the line of serpentine outcrops?

5. Dr. Frazer says: "If there be any allusion to the Serpentine belt of Radnor and Easttown on page 33,† it is so covert that I fail to perceive it. * * * Page 34, being a map, naturally contains no assertion of any kind." I quote from page 33: "My view of a section along a line from Bryn Mawr northwest to a point in the north line of Radnor township, * * * that is west of Mr. Hall's line H, * * * is given herewith, * * * 1. Syenite. Northward. 2. *Serpentine.* * * * I submit herewith a map showing most of the outcrops mentioned." On the next page is the map, showing among other things the outcrops of the Radnor and Easttown Serpentine belt on the northerly side of the great Syenite anticlinal. A map may not be an "assertion," but if one publishes a geological map, and upon it indicates clearly the outcrop of a certain rock, it is a fair inference that he meant that such a rock was in the locality marked.

6. Dr. Frazer insists on the accuracy of his date for the publication of C_6,

* Proc. Min. and Geol. Soc. Acad. N. S., No. 1, p. 93.
† Rand, Proc. Min. and G. Sec., 1880, 1881, p. 33.

viz., "late in 1882."* I stated† that that volume was in the library of the Academy, and in my possession, in 1881; that my criticism of it was read in December, 1881, and published in April, 1882; which statements, if the book was not published until late in 1882, as Dr. Frazer again asserts, cannot be true. Until now I supposed his remarks in the October *Naturalist* were intended to explain how he fell into the error, and not to affirm it; but his present statement is so clear that I feel compelled to publish the following to relieve myself of the odium of such a charge:

HARRISBURG, PA., Nov. 30, 1881.

THEO. D. RAND, Esq., 17 South Third street.

DEAR SIR:—At the request of Mr. Murray Graydon, * * * I send you by mail to-day 1 copy of Report C_6. * * * * * *

Very truly, FRED'K W. FORMAN.

[Copy.]

Second Geological Survey of Pennsylvania.

J. PETER LESLEY, State Geologist,
1008 Clinton street, Philadelphia.

MARCH 13, 1884.

Mr. THEO. D. RAND.

DEAR SIR:—C_6 was published, I think, in September, 1881; but, as the publication office at Harrisburg was under the direction of the Secretary of the Board, and as his clerk is no longer in the service of the Survey, I am unable to give a more accurate date; indeed, it would be impossible for any one to fix a date.

Yours truly, J. P. LESLEY, per H.

Equally with Dr. Frazer do I deprecate the "firing at long range." For this very reason, I, pursuant to a published promise, produced the rocks themselves at the meeting of the Mineralogical and Geological Section of the Academy, hoping that the points of difference might be there amicably discussed and the truth elicited. It is the right of every observer to set forth his observations, even though they may differ from the views of those engaged in the survey of the State.

THEO. D. RAND.

Regarding which Dr. Frazer rejoins:

"With one printing press and one official channel open to him, which are closed to me, it is clear that Mr. Rand can print more words in a given time. The above paragraphed reply seems to me to be characterized by 'undistributed middle,' not to say pettifogging. For instance, No. 2 is an obstinate attempt to put in the mouth of another what that other never said, and has denied having ever meant. In the face of such a fact the effort to fasten upon my words such repudiated meanings is quite unfair. Such phrases as 'naturally import' (No. 2), the comment on Prof. Les-

* Frazer, Am. Nat., May, 1883, p. 523.
† Am. Nat., September, 1883, p. 966.

ley's language (No. 2),* 'fair inference' (No. 5), and the quibble about the chlorite in the hydromica schists, are purely gratuitous constructions which it pleases Mr. Rand to put on language which will not sustain them.

"Nos. 3 and 5 appear to me to be the veriest trifling with words.

"No. 4 is an entire misstatement of my clearly expressed views. Though the 'talc schist' contain no talc, and therefore no magnesia, it does not follow that in the series there are no layers of magnesian rocks which are *not* 'talc-like.'

"In C_3 pp. 268† +, several analyses of such rocks remote from the serpentine outcrops will be found.

"(6) I *did* insist that C_6 was published October 24, 1882, for the reason that on writing to the clerk of the Board of Commissioners, Mr. F. W. Forman, who was the official charged with receiving, issuing, and recording the date of issue of reports, I received and have still a letter dated September 8, 1883, in which the dates of issue of all the reports up to that time are given, and that of the issue of C_6 is October 24, 1882. If Mr. Forman was mistaken I am not to blame, because having been in Europe during the entire period between Mr. Forman's date and Mr. Rand's I took every precaution to ascertain with accuracy what I stated. The State Printer, however, in answer to a letter from me, fixes the date of the delivery of 1,400 copies of C_6 to Mr. Forman as October 24, 1881."

PERSIFOR FRAZER.

Commenting upon the preceding, Mr. Rand adds:

"Of the analyses C_3, p. 268, two are of rock from *York* county. The third is in Lancaster, as follows; the ?? are in the original:

"No. 977, Variegated Chlorite Schist with Chlorite (?) half a mile northeast of Pine Grove.

Silica ($Si O_2$)	37·03
Titanic Oxide ($Ti O_2$)	4·05
Phosphoric Oxide ($P_2 O_5$)	0·51
Alumina ($Al_2 O_3$)	24·13
Lime ($Ca O$)	·21
Magnesia ($Mg O$)	1·44
Ferrous Oxide (?) ($Fe O$)	19·83
Potash, trace soda	8·93
Ignition	3·54

"This needs no comment. The Map C_3 shows the locality in the line of strike of the Texas Serpentine, and but about two miles distant."‡

THEO. D. RAND.

---

* The added words only prove more conclusively than ever that the quotation could not justify Mr. Rand's supposition that C_4 denied the existence of quartzite on any part of the southern side of the Chester valley, and that Lancaster and Chester were alone under consideration. Lancaster, up to the point where my citation stopped, and by a natural transition of thought in the portion continued by Mr. Rand, Chester

† 268 + means p. 268 and succeeding pages. Mr. Rand asked for an analysis of specimens (of hydro-mica schists) not near a serpentine outcrop. I referred to p. 268, where there are three analyses of the associated chloritic layers; + (*i. e.*, p. 270) Peach Bottom slate, (and p. 271,) mica schist. His remark about 977, (a specimen from the west slope of the South Mountain in Cumberland county,) which follows this, I cannot understand. His assertion as to the locality is some fifty-six miles in error, the horizon is probably the same as that of the Chester schists and magnesia is present.

‡ Dr. Frazer's note leads me to think there may be two *Pine Groves*. If fifty-six miles from Pine Grove on the Octorara it is equally with York county out of the region discussed.

Distribution of the Heat which is Developed by Forging.—On the 8th of June 1874, Tresca presented to the French Academy some considerations respecting the distribution of heat in forging a bar of platinum, and stated the principal reasons which rendered that metal especially suitable for the purpose. He subsequently experimented, in a similar way, with other metals, and finally adopted Senarmont's method for the study of conductibility. A steel or copper bar was carefully polished on its lateral faces, and the polished portion covered with a thin coat of wax. The bar thus prepared was placed under a ram, of known weight P, which was raised to a height H, where it was automatically released so as to expend upon the bar the whole quantity of work $T=PH$, between the two equal faces of the ram and the anvil. A single shock sufficed to melt the wax upon a certain zone and thus to limit, with great sharpness, the part of the lateral faces which had been raised during the shock to the temperature of melting wax. Generally the zone of fusion imitates the area comprised between the two branches of an equilateral hyperbola, but the fall can be so graduated as to restrict this zone, which then takes other forms, somewhat different, but always symmetrical. If A is the area of this zone, b the breadth of the bar, d the density of the metal, c its capacity for heat, and $t-t_0$ the excess of the melting temperature of wax over the surrounding temperature, it is evident that, if we consider A as the base of a horizontal prism which is raised to the temperature t, the calorific effect may be expressed by

$$Ab \times d \times C(t-t_0)$$

and on multiplying this quantity of heat by 425 we find, for the value of its equivalent in work,

$$T^1 = 425\, AbdC(t-t_0).$$

On comparing T^1 to T we may consider the experiment as a mechanical operation, having a minimum of

$$\frac{T^1}{T} = \frac{425}{PH} AbdC(t-t_0).$$

After giving diagrams and tables to illustrate the geometrical disposition of the areas of fusion, Tresca feels justified in concluding that the development of heat depends upon the form of the faces and the intensity of the shock; that the points of greatest heat correspond to the points of greatest flow of the metal and that this flow is really the

mechanical phenomenon which gives rise to the calorific phenomenon; that for actions sufficiently energetic and for bars of sufficient dimensions, about $\frac{8}{10}$ of the labor expended on the blow may be found again in the heat; that the figures formed in the melted wax, for shocks of less intensity, furnish a kind of diagram of the distribution of the heat and of the deformation in the interior of the bar, but that the calculation of the coefficient of efficiency does not yield satisfactory results in the case of moderate blows.—*Comptes Rendus*, July 23, 1883.

Strength of Portland Cement.—The Portland cement, as it comes from the factory, is composed of an almost impalpable powder, mixed with coarser grains which have but little adhesive quality. Mann's experiments have shown that in a trial of seven days the portions which pass through a No. 176 sieve (31,000 meshes per square inch) exhibit five times the adhesive strength of those which pass through a No. 103 sieve (10,600 meshes per square inch). The necessity of fine grinding is therefore obvious. In ordinary cement 45·6 per cent. is stopped by the No. 176 sieve, which is the finest that is made. The force of cohesion is much greater than that of adhesion, varying from threefold to tenfold. The adhesive force upon different substances, such as stone, brick, slate, marble, glass, etc., varies greatly. The degree of surface polish has less effect than one would think. According to these experiments, the best test of Portland cement is its adhesive power. The No. 176 sieve ought not to stop more than 45 per cent. of the cement; the part which traverses the sieve should have an adhesive force of 95 pounds, and the unsifted cement of 75 pounds per square inch.—*Chron. Industr.*, Sept. 30, 1883. C.

Tripolith.—A substance has been manufactured in Heidleberg, for two years, which is called *tripolith*, and which is prepared as follows: three parts of an argillaceous gypsum are mixed with one part of silicious clay, and nine parts of this mixture are added to one part of coke. The entire mass is placed in a caldron, without water, and is heated, with continual stirring, until it reaches the temperature of 120° (248°F.), in order that the water of crystallization may be expelled from the gypsum. The whole is then heated to 260° (500°F.), and subsequently pulverized and sifted. The tripolith is harder, tougher and lighter than gypsum, and is better adapted to the manufacture of stuccos and the decoration of columns, etc.—*Gaceta Industrial*, Sept. 25, 1883. C.

Manufacture of Soda from Sea Salt—The municipality of Issoudun, having resolved to erect a monument to Nicholas Leblanc, the pioneer in the artificial soda industry, M. Dumas gave an eloquent address to the French Academy upon the importance and vicissitudes of that branch of manufacture. A hundred years ago the French Government consulted the Academy as to the best means of replacing the soda supply, for which they had been dependent upon Spain, and a prize of 12,000 francs was offered to the inventor of a successful process for extracting the alkali from sea salt. When Leblanc had fulfilled the conditions of the prize the Academy had ceased to exist; the inventor was obliged to renounce his rights, to close his factory and to live in the extreme of penury, until finally he committed suicide. Many persons would be surprised at the information that the two greatest economical novelties of the century are the steam engine and artificial soda, the two most fertile inventors, Watt and Leblanc. While the engines created by the former act with a great noise in all our factories, carry the trains of travelers and merchandize over the iron roads with which the continents are furrowed, or guide ships of commerce and war over the waves of the sea, the soda products penetrate into all our workshops and dwellings, as indispensable elements or auxiliary agents of labor, and as direct or indirect objects of consumption. If we were asked which of the two inventors, Watt or Leblanc, has most greatly increased the welfare of our race, we might hesitate for an answer. All the ameliorations in the mechanic arts spring, it is true, from the use of the steam engine; but all of the benefits which pertain to the chemical industries have found their point of departure in the extraction of soda from sea salt. The first result of the soda factories placed at the disposal of the soap makers, the glass manufacturers, the bleachers and the paper makers the alkali of which they have need, and also offered to all industries sulphuric and chlorhydric acid, in unlimited quantities and at prices fabulously reduced. The second result was the introduction of chloride of lime for the rapid bleaching of vegetable tissues, in place of the slow action of solar light and damp air upon pieces of linen spread upon the grass. These four agents—a powerful alkali, two energetic acids, a bleaching powder which nothing has supplanted—gave an unprecedented range to chemical industries, and raised the question whether it was wise to leave the fabrics dependent upon the sulphur of Sicily, which might, at any moment, be enormously advanced in price; and iron pyrites,

which had previously been almost worthless, was largely employed in the manufacture of sulphuric acid. The growth and competition of large soda factories reduced the price until it became necessary to look for new sources of profit, which were found, for awhile, in the manufacture and sale of chloride of lime. The leaching of the crude soda to extract the carbonate left residues, which contained all the sulphur of the sulphuric acid united to the lime. This refuse incommoded the whole neighborhood, infecting the water courses and the shores of the sea itself. These annoyances were removed by the invention of a process for regenerating the soda which was left from the leachings. The manufacture of chlorine and chloride of lime consumed peroxide of manganese, and produced chloride of manganese in great quantities. The peroxide is a natural product of limited supply; to increase its consumption is to raise its price. The chloride destroys vegetation and infects the streams; a considerable daily production of it creates a thousand difficulties. They have been obviated by a process for regenerating the peroxide and thus getting rid of the chloride. Meanwhile, the competition continuing, and the price of soda steadily falling in proportion as the cost of manufacture diminished, help was sought, not in new economies, but in the treatment of ores capable of furnishing remunerative merchantable products. Hence the iron pyrites was replaced by copper pyrites which were accompanied by precious metals, and the profit was sought in the silver or gold which could be extracted from their cinders. This strife is now to be renewed with a rival process founded upon the decomposition of salt by ammonia in presence of an excess of carbonic acid. Whatever may be the result of the contest, it is fortunate that modern chemistry has had the various practical schools of industry which have resulted from Leblanc's process, and which have exercised an incalculable influence upon all civilized countries.—*Les Mondes*, Aug. 11, 1883. C.

Dangers in the Use of Earthenware.—E. Peyrusson, having been called upon to examine some pottery which was suspected of having produced lead-poisoning, found that, in spite of the ministerial circulars, the glazing often contains lead enough to make it dangerous for use. M. Constantin has invented a process which is entirely harmless as well as economical—varnishing with boro-silicate of lime. The glazing of fine French and English china has been much improved by the addition of boric acid and borate of lime, which allows a great

diminution in the quantity of ceruse employed; but even their habitual use may occasion injury by the accumulation of small quantities of lead in the system. There are always cracks in the glazing even of the finest china after it has been used for some time. These cracks may retain, in spite of the most careful washing, germs of fermentation, as is shown by the fact that milk or broth will ferment more readily in a vessel which has once been soured than in one which is new. The analogy between fermentation and contagious diseases gives room to fear that earthen vessels which have been used by the sick may carry contagion to others.—*Les Mondes*, Aug. 25, 1883. C.

New Mining Powder.—According to the *Annales Industrielles*, M. Michalowski, an engineer at Montceau-les-Mines, has just invented a new explosive. It is a powder of a density little more than half as great as that of ordinary powder, with irregular grains of a slate-gray color. It does not explode by the action of fire, and detonates only under a blow, like dynamite.—*Les Mondes*, Aug. 25, 1883. C.

Book Notices.

TRAITÉ PRATIQUE D'ÉLECTRICITÉ COMPRENANT LES APPLICATIONS AUX SCIENCES ET L'INDUSTRIE, ETC. Par C. M. Gariel. Tome Premiere. Avec 253 figures dans les texte. Paris: Octave Doin, Editeur. 1884.

The intention of the author is at once to avoid the classic routine of the ordinary text-book on electricity, and to present the more important developments of electrical science in such form as may be particularly instructive to the general or non-technical reader. Instruction, as the main object, is followed with honest plodding purpose, to the exclusion of much that might scientifically amuse, and we may say, as we pick this book out of the present frothing flood of light literature on electricity, that it is quite above the average popular work in tone and in execution.

While freshness and point is given to the treatment by keeping the applications steadily in view, the author does not hesitate to explain many essential theoretical matters omitted from works of a more technical pretension. Asserting that it is not necessary in a work of applied physics that the facts should disappear "derrière les equations," the

author for the most part avoids mathematical formulæ. And yet the few formulæ employed are happily chosen, and also clearly show that there is a limit to the popular treatment of even applied science which, for the sake of science and of the applications, it is well to recognize.

We are particularly pleased to find in a popular work chapters on magnetic and electrical measurement so neatly done, and we note throughout the author's clear apprehension of the essentials of electrical science. M. B. S.

DYNAMIC ELECTRICITY, including, I, Some Points in Electric Lighting, by Dr. John Hopkinson, F. R. S.; II, On the Measurement of Electricity for Commercial Purposes, by James N. Schoolbred, M. Inst. C. E.; III, Electric Light Arithmetic, by R. E. Day, M. A. Science Series, No. 71. New York: D. Van Nostrand.

This little volume will be read with interest by those engaged in the practical manipulation of dynamic electricity. The joint issue of three essays like these does not detract from either, and may prove an advantage. The first treats more particularly of the theory of the dynamo circuit, the second of the practical measurement of electrical effect, and the third contains the numerical solution of some problems arising in the daily practice of electric lighting. M. B. S.

THE INTERNATIONAL ELECTRICAL EXHIBITION.

[To open, Tuesday, September 2d, 1884; to close, Saturday, October 11th, 1884.]

In the JOURNAL for September, 1883, there appeared a general announcement of the intention of the Franklin Institute of the State Pennsylvania for the Promotion of the Mechanic Arts, to hold an International Electrical Exhibition in the autumn of the year 1884, and giving the text of the Act of Congress relating thereto, which provided for the admission into the United States, duty free, of all articles intended for the exhibition.

Since this publication was made, the committee on exhibitions has been actively engaged in the work of organizing and perfecting the plans. At the present time much of this preliminary work has been completed, and that remaining to be done is in as advanced a state as time and circumstances have permitted.

Briefly summarized, the steps that have been taken are the following: A suitable site for holding the exhibition has been obtained in

the large vacant lot bounded by Thirty-second and Thirty-third streets, Lancaster avenue and Foster street, which, by the liberal action of the Pennsylvania Railroad Company, has been leased to the Institute for the purposes of the exhibition for a nominal consideration.

The accompanying plate (see frontispiece) is a view of the exhibition building, which is now in process of erection, and which, by the terms of the contract, will be finished by the 15th of June. The building is being erected by Mr. Jacob R. Garber, from the plans of the architects, Messrs. Wilson, Brothers & Co.

The following brief description will give a general idea of its character:

The main building will be rectangular, having a length on Foster street of 283 feet and a breadth of 160 feet, extending from Foster street to Lancaster avenue on Thirty-second street, and part of the distance from Foster street to Lancaster avenue on Thirty-third street. A tower sixty feet high will be situated at each of the four corners of this building. One central arch of 100 feet span and 200 feet in length, of the Gothic style of architecture, will cover the greater portion of the space occupied by this building, while two smaller ones, having a span of thirty feet and running parallel to it on either side, will join the towers. The building will have second story apartments at its ends on Thirty-second and Thirty-third streets respectively, with stairways leading up in the towers from the ground floor. The towers themselves will be three stories high. Two long and narrow hallways will afford communication between these apartments. The remainder of the ground will be enclosed by a large triangular building one story in height and joined to the main hall. The main entrance to the building will be at the corner of Thirty-second street and Lancaster avenue, another at Thirty-third and the avenue, and one at each of the other towers. Five exits are provided for on the plans, but desirable changes may hereafter be made in the number and situation of both entrances and exits before the work is completed.

The meeting of the American Association for the Advancement of Science, which will be held this year in Philadelphia, and the expected presence of many representatives of the British Association, which will meet this year in Montreal, will attract a numerous and influential scientific gathering in Philadelphia during the time of the holding of the exhibition; and in order that so exceptional an opportunity to promote the interests of science shall not be lost, Congress has been

requested to authorize the holding of a National Conference of Electricians, to convene in Philadelphia at this time. Should Congress, in its wisdom, make the proper provisions for holding such a conference, the results promise to be of much value.

The scientific character and importance of the exhibition have been duly appreciated by the United States Goverment, and the following official letter issued by the Department of State, conveying instructions to our diplomatic officials abroad, is at once as complete an endorsement of the project as could be given, and a flattering tribute to the reputation and usefulness of the Franklin Institute.

[COPY.]

DEPARTMENT OF STATE,
Washington, March 1, 1884.

SIR:—I transmit herewith printed copies of a circular issued by the FRANKLIN INSTITUTE of the State of Pennsylvania, respecting an International Electrical Exhibition to be held at Philadelphia, from September 2d to October 11th, 1884.

Although the exhibition will be organized and conducted apart from the immediate control of the United States, the advantages for useful results, which a direction by the FRANKLIN INSTITUTE confers, have been distinctly recognized by Congress. The Joint Resolution of February 26th, 1883, extended the privilege of "free entry" to all articles from abroad which shall be intended and used for the purpose of the exhibition, and precise instructions have in consequence been formulated for the guidance of Customs officers.

It is likewise proper that the offices of our representatives abroad should be engaged in the same interest to the extent of bringing the project officially to the attention of the governments to which they are respectively accredited, and, by all suitable methods, of making known to individuals who are or might be interested, the character, standing and profession of the exhibition above designated.

With the object of obtaining from you the service just indicated this instruction is addressed.

I am, sir, your obedient servant,

(Signed.) FRED'K T. FRELINGHUYSEN.

A comprehensive scheme of classification has been carefully elaborated; a system of rules and regulations to govern the internal management of the exhibition has been adopted; provisions have been made in the interest of intending foreign exhibitors, to relieve them of all trouble in respect to the passage of their exhibits through the Custom

House, and for the proper reception and care-taking of the same on their arrival; and arrangements have been made with a number of the leading transportation companies, to return, free of charge, goods on which freight charges have been paid one way.

The above information, expressed in detail, has been published in the form of a twelve-page pamphlet, which, with a blank form of application for space has been issued in the English, French and German languages, and extensively circulated in the United States and throughout Europe.

There are evidences at this time even, to indicate that the exhibition will be one of unusual interest and value. The active participation of several of the scientific bureaux of the United States Government and of all the leading electrical companies is assured. Numerous inquiries both from official and private sources have been received from abroad, and interesting and valuable contributions from European countries are confidently anticipated.

The circular of information herein referred to, with blank forms of application for space, may be obtained in the English, French or German languages, by addressing a request therefor, to the Secretary of the Franklin Institute. W.

Franklin Institute.

[*Proceedings of the Stated Meeting, Wednesday, March* 19, 1884.]

Hall of the Institute, March 19, 1884.

The meeting was called to order at the usual hour, with the President, Mr. Wm. P. Tatham in the chair. There were present 168 members and 12 visitors. The minutes of the February meeting were read and approved. The Actuary submitted the minutes of the Board of Managers, and reported that at the stated meeting held Wednesday, March 12th, 1884, 19 persons had been elected to membership.

The Secretary reported that, pursuant to instructions, he had caused the proposed amendment to Article III, Section 3, of the By-Laws, offered by Mr. Sartain, at the last stated meeting, to be advertised weekly in two of the daily newspapers. The amendment in question proposes the addition to Article III, Section 3, of the words "*provided*, that no payment less than the full contribution for one year shall